THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

THE EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

REGIUS PROFESSOR OF NATURAL HISTORY, LECTURER ON MINERALOGY, AND KEEPER OF THE MUSEUM IN THE UNIVERSITY OF EDINBURGH;

Fellow of the Royal Societies of London and Edinburgh; Honorary Member of the Royal Irish Academy; of the Royal Society of Sciences of Denmark; of the Royal Academy of Sciences of Berlin; of the Royal Academy of Naples; of the Geological Society of France; Honorary Member of the Asiatic Society of Calcutta; Fellow of the Royal Linnean, and of the Geological Societies of London; of the Royal Geological Society of Cornwall, and of the Cambridge Philosophical Society; of the Antiquarian, Wernerian Natural History, Royal Medical, Royal Physical, and Horticultural Societies of Edinburgh; of the Highland and Agricultural Society of Scotland; of the Antiquarian and Literary Society of Perth; of the Statistical Society of Glasgow; of the Royal Dublin Society; of the York, Bristol, Cambrian, Whitby, Northern, and Cork Institutions; of the Natural History Society of Northumberland, Durham, and Newcastle; of the Imperial Pharmaceutical Society of Petersburgh; of the Natural History Society of Wetterau; of the Mineralogical Society of Jena; of the Royal Mineralogical Society of Dresden; of the Natural History Society of Paris; of the Philomathic Society of Paris; of the Natural History Society of Calvados; of the Senkenberg Society of Natural History; of the Society of Natural Sciences and Medicine of Heidelberg; Honorary Member of the Literary and Philosophical Society of New York; of the New York Historical Society; of the American Antiquarian Society; of the Academy of Natural Sciences of Philadelphia; of the Lyceum of Natural History of New York; of the Natural History Society of Montreal; of the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanical Arts; of the Geological Society of Pennsylvania; of the Boston Society of Natural History of the United States; of the South African Institution of the Cape of Good Hope; Honorary Member of the Statistical Society of France; Member of the Entomological Society of Stettin, &c. &c. &c.

Jan 1851 ~~OCTOBER 1850~~ .... APRIL 1851.

VOL. L.

TO BE CONTINUED QUARTERLY.

EDINBURGH:

ADAM AND CHARLES BLACK.

LONGMAN, BROWN, GREEN, & LONGMANS, LONDON.

1851.

EDINBURGH:
PRINTED BY NEILL AND COMPANY, OLD FISHMARKET

CONTENTS.

CONTENTS.

THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL.

Address delivered at the Anniversary Meeting of the Geological Society of London, on the 15*th February* 1850. By the President, Sir Charles Lyell, F.R.S., &c. &c. *

Gentlemen,—It is now my duty, in accordance with the usual custom of my predecessors in office, to say something of the scientific labours of geologists during the past session. It is nearly twenty years since I announced, in the first edition of my "Principles of Geology," the conviction at which I had then arrived, after devoting some time to observation in the field, and to the study of the works of earlier writers, that the existing causes of change in the animate and inanimate world might be similar, not only in kind, but in degree, to those which have prevailed during many successive modifications of the earth's crust. I attempted to adapt the views which Hutton and Playfair had first promulgated, to a more advanced state of our science, and to extend their application, by shewing, that should the same causes continue to act with unabated energy, for indefinite periods of the future, they must bring about revolutions not inferior in magnitude to those recorded in the monuments of past ages. After an interval of twenty years, during which Geology has been enriched by a vast accession of new facts, and when so many powerful minds, in every civilized country, have brought their intellectual energies to bear on the philosophy of our science, I may I think affirm that the idea of comparing the modern agents of change with those of remote epochs, as not inferior in power and intensity, appears even

* Copy communicated by the Author.

to the most sceptical a far less visionary and extravagant hypothesis than when I first declared my belief in its truth. As, however, there are not a few original observers, whose opinion I respect, who are still opposed to this doctrine, I cannot I believe do better on the present occasion than take a brief view of the bearing of some leading discoveries of modern date on this much-controverted question. I adopt this course the more willingly, because a perusal of the memoirs read before the Society during the past session, and the contemporary publications of other scientific bodies and authors in Europe and America, has convinced me that they are so varied and so overwhelming by their number and importance, as to make it impossible, within the limits of this anniversary Address, to give an analysis of the contents of each, still less to add criticisms and comments of my own. But in order to keep myself still further within due bounds, I shall not enter at present the field of palæontology, reserving for a future opportunity a comparison of the organic creation, in ancient and modern times, and the question whether the fluctuations of the living inhabitants of the globe have been regulated formerly by the same laws as now.

Among the points of geological interest relating exclusively to the inanimate world, none have given rise to a greater difference of opinion than the various causes suggested to account for the position of stratified and unstratified rocks in mountain chains. They are usually referred to the development of mechanical and volcanic forces of a paroxysmal character; but geologists who favour these views are by no means agreed whether the causes thus capable of modifying the earth's crust, were all of them in the beginning in a state of the highest intensity, and afterwards declined in energy; or whether they have been exerted again and again during short intervals of violent convulsion, followed by long periods of repose. On these, and questions of a kindred nature, I shall proceed to offer some observations, well aware that I shall advocate opinions which I have long cherished, and on which I can scarcely fail to have a strong bias; but reminding you at the same time, that they who defend conclusions opposed to mine, have equal reason to doubt their own impartiality, and to suspect that they also may be influenced by old associations, and those strong prepossessions, with which nearly all the early literature of our science is imbued. It may be true that no geologist worthy of the name would contend at this time of day for the modern origin of our planet, or maintain the doctrine that it was created contemporaneously with man, although the multitude, including many of the educated classes, may, in their gnorance of the records of creation as written in the heavens and the earth, still fondly cling to such opinions. The cultivators of our science may be ready to grant the most indefinite duration to each successive geological epoch, yet they may still unconsciously derive a love of cataclysms and catastrophes, and faith in a primæval chaos out of which the present order of things was evolved, from an hereditary creed, not founded on facts, or strict inductive reasoning on natural phenomena.

As introductory to this subject, I cannot do better than recal your attention to the recently published memoir of Sir Roderick Murchison, on the Structure of the Alps, Appenines, and Carpathians, which deservedly occupies an entire Number of your Quarterly Journal.* It comprises a masterly summary of the labours of those who had gone before him, in a very difficult field of inquiry, as well as a luminous account of his own personal investigations, and should be studied by every one who is desirous of knowing what point the modern progress of geology has reached. On various important questions of which he treats, and in which I entirely agree with him, I cannot enter at present, but there is one leading conclusion established in his memoir which bears specially on the theory selected for discussion in this Address. He proves, as it appears to me in a satisfactory manner, that those stupendous movements to which the loftiest chain in Europe owes its complicated structure, and by which its component strata have been dislocated, fractured, and contorted, belong to a very modern era in the earth's history. In the long calendar of geological events, the Eocene period is the first

* Vol. v. Part i. 1848, December.

which presents us with a fossil flora and fauna, both terrestrial and aquatic, of a very complete character, comprising mammalia both of the sea and land, of all the principal classes, now contemporary with man. It would doubtless be rash to assume that no plants or animals of equally high organization may not have pre-existed on this globe, for the recent progress of discovery in our science puts us on our guard against founding hasty generalizations on mere negative evidence. The fossil skeletons of saurians discovered in the coal-measures of Saarbrück near Treves are still fresh in our recollection, as are those footprints of the same age first detected by Dr King, and which I have myself examined at Greensburg in Pennsylvania. We are waiting also with impatience for more minute details respecting some reptilian footprints of a still more ancient date, found by Mr Isaac Lea in the old red sandstone at Pottsville, near Philadelphia; nor have we forgotten the tracks of numerous birds, observed in the red shales and sandstones of Connecticut, of a date nearly bordering on palæozic times. Such facts, like the unexpected discovery of the Stonesfield marsupials, a quarter of a century ago, warn us against the presumption of taking for granted, that our present knowledge of the earliest occurrence of a particular class of fossils in stratified rocks, can be reasoned upon as if it afforded a true indication of the first appearance of a particular class of beings on the globe. Nevertheless, with every reservation for the future enlargement of our ideas respecting the comparative perfection of the living creation in our own times and in the remoter ages, we may at least assert, that in the present state of our science the eocene fauna and flora may be contrasted with those of older date, in regard to the more complete manner in which they represent the animal and vegetable creation.

In the chronological classification of the materials composing the crust of the earth, it has been often asked, whether we ought to ascribe to the older tertiary epoch, or to the cretaceous system, the great nummulitic formation of the Alps, and other parts of Europe. This much-controverted question,—one, as I shall presently point out, of the highest theoretical interest, in reference to the hypothesis of the unabated intensity of the existing agents of change,—was declared by M. Boué, some years ago, to be the great problem of the day, and Sir R. Murchison has therefore devoted to its consideration a large portion of his memoir. M. Boué indeed announced in 1847 his own conviction that the nummulitic rocks belonged to the eocene or lower tertiary period, and remarked, in a paper read to the French Geological Society in that year, how much delight Alexander Brongniart would have experienced, had he lived to see one of his boldest and most startling generalizations thus crowned with success.* Alexander Brongniart had in fact declared many years before, that the shells of the summit of the Diablerets, one of the loftiest of the Swiss Alps, which rises more than 10,000 feet above the sea, were referable to species characteristic of the eocene strata of the neighbourhood of Paris. He only felt considerable hesitation, he said, in assigning to them so modern a date, because the overlying limestones were so compact and homogeneous as to agree in lithological character with much older secondary rocks.

Several of the most animated discussions which have taken place in this room since 1825, have turned, as you will recollect, on this subject, especially when the fossil shells brought by Mr Pratt from Biaritz in the Pyrenees were laid upon our table. A decided opinion was then expressed by many of us that the nummulitic series of that southern chain must be referred to the lower part of the eocene group, as it was made clear that the proportion of fossil species common to the Biaritz beds and the chalk was extremely small—much too few to imply a cretaceous age for the strata in question, or even a zoological passage from the cretaceous to the tertiary formations. They who have read with care the successive numbers of the "Bulletin" of the Geological Society of France, are aware how much that body has been occupied with the same problem, and how steadily the evidence in favour of the same important conclusion has been gaining strength. M. d'Archiac, writing in 1847 on the fine collection of the Biaritz shells submitted to his inspection by Mr Pratt, observed that forty-

* Bulletin, vol. v. 2d Series, pp. 69, 71.

eight, or one-fourth of the whole series, were identical with fossils of the lower eocene of the Paris basin, while the rest were all tertiary forms except four, which belonged to species of the chalk.* In a paper by M. Deshayes, read to the Geological Society of France in June 1844,† that able conchologist declared, after examining the Biaritz fossils, "that the whole of the nummulitic system must be classed as tertiary; an opinion confirmatory," he said, "of the results previously arrived at by M. Leymerie in the Corbières, and of M. Bertrand Geslin in the Alps. Lastly, I may observe, that you will find similar opinions recorded in the "Bulletin," either in the memoirs or verbal comments of MM. Deshayes, Charles Desmoulins, Raulin, Leymerie, Tallavigne, Delbos, Desor, Boué, Archiac, and Alcide D'Orbigny, all published in the course of the last six years. Whether a real transition from the cretaceous to the tertiary strata can be made out, is a point which has also been fully discussed, and how far the Maestricht beds are represented in the Pyrenees. It appears from the researches of MM. Desmoulins and Raulin, that some few of the characteristic fossils of Maestricht have really been found in that chain; but you will, I think, agree with M. Deshayes, that they are not enough to establish the existence of any true equivalent of the Maestricht group —that distinct and uppermost division of the chalk to which the Faxoe coralline limestone in Seeland, as well as the pisolitic strata of Sezanne near Paris, are referable.

When we consider that the age of the nummulitic formation of the Pyrenees, however clearly it may now be determined to be tertiary, has been regarded by so many able authorities as a subject of perplexity and debate up to so late a period, we cannot feel surprised that MM. de Beaumont and Dufrénoy, in constructing their geological map of France many years before, should have referred these strata in the Alps, and in the regions bordering the Mediterranean, to an age anterior to the calcaire grossier of Paris, especially when we learn that even now M. Agassiz affirms, that out of

* Bulletin, vol. iv. 2d Series, p. 1006.

† Translated in Quart. Journ. Geol. Soc. 1845, p. 111.

139 species of echinoderms described by him from the nummulitic beds of the Mediterranean, one species only is common to them and the calcaire grossier. The same geologist maintains that all the fish of Glarus and Monte Bolca, which according to the latest opinions must be classed as eocene, differ entirely from those of Sheppy.* Yet I am by no means disposed to question, on the ground of this want of agreement in the ichthyolites, that the Glarus slates are in truth tertiary, still less to doubt that the limestone of Monte Bolca belongs to the same period : I have always regarded the latter as eocene from the time when I visited that locality in company with Sir Roderick Murchison in 1828. You have seen also, in the classification of the three successive eocene formations established by Mr Prestwich for the older tertiary deposits of Great Britain, that while each division is characterized by its peculiar assemblage of shells, a part only of the species pass from one division to another, and that the specific difference of the mammalia belonging to each division, and still more of the first, as determined by Agassiz, is extremely marked.

The researches above alluded to, of Sir Roderick Murchison in the Alps in 1847, and the palæontological evidence of various eminent writers brought together by him in illustration of his views, have, I think, shown unequivocally, that, together with the nummulitic limestone, an enormous thickness of overlying strata of dark-coloured slates, marls, and fucoidal sandstones, provincially called Flysch, are separable from the cretaceous system of Northern Europe, and must also be regarded as lower eocene. His attempt however to make out a passage from the tertiary to the secondary series by means of an intervening group of marls, green sandstone, and impure limestone, appears to me to be far less successful, since a true representative of the Maestricht beds is wanting in the Alps, or is very ill-defined, and no other equivalent assemblage of organic remains is enumerated sufficiently rich in forms, or intermediate in character, to fill up the wide gap between the eocene strata and the chalk.

---

* Bulletin, vol. v. pp. 414, 415.

I have dwelt thus at length on the age of the nummulitic series, because its recognition as a tertiary deposit draws with it consequences of the utmost theoretical importance, and is singularly confirmatory of a remark made by M. Desnoyers many years ago in his address to the French Geological Society, namely, "that the more the Alps are studied the younger they grow." This saying was elicited by the admission by competent observers, that certain schistose rocks of great thickness, containing dark writing slates, originally classed as "transition formations" by some of the followers of Werner, and regarded as of palæozoic age, were really secondary. Now we are called upon to go much further; for these same strata belong to the flysch, and therefore constitute what is by no means the base of the eocene system. To the English geologist who is old enough to remember when all the soft clays and loose sands overlying the chalk, some of them containing shells of species identical with those now living, were looked upon as very modern, and as the creations of yesterday, in comparison with the rocks of the higher Alps, it may well appear a startling proposition to learn that the clay of London was in the course of accumulation as marine mud at a time when the ocean still rolled its waves over the space now occupied by some of the loftiest Alpine summits. It will follow, moreover, as a corollary from the same date, as before hinted, that not only the upheaval of the Alps, but all the principal internal movements, dislocations, inversions, and contortions of the strata, are subsequent to the origin of the nummulitic deposits, and had not therefore even commenced till great numbers of the eocene vertebrate and invertebrate animals had lived and died in succession.

If the development of so vast an aggregate amount of dynamical agency in times so modern in the earth's history had been confined to a single narrow zone of mountains, it would be a fact of no small significance as invalidating all theories which ascribe such magnificent displays of mechanical force to very remote epochs. But on extending our survey, we find some of the members of this nummulitic series, with their characteristic fossils, playing the same part in the Pyrenees, Apennines, and Carpathians, and spreading over a large part of the globe of which the geology is best known. They are met with in full force in the north of Africa ; as for example in Algeria and Morocco ; they have been traced from Egypt into Asia Minor, and across Persia by Bagdad to the banks of the Indus. They occur not only in Cutch, but in the mountain-ranges which separate Sinde from Persia, and which form the passes leading to Cabul. They have been followed still further eastward into India, and may be said to enter bodily into the structure of all the continental lands and mountain-chains of the Old World.

Were we to endeavour to estimate the changes in physical geography which can be proved by the position of these marine eocene strata to have occurred since the commencement of the tertiary period, we should find them to be very inadequately expressed by stating that they equal in amount the conversion of sea into land of a continent as large and lofty as that of Europe, Asia, and the north of Africa. I endeavoured in 1834, in a map constructed for the 3d edition of my " Principles of Geology," to show the extent of surface in Europe and part of Asia which had been covered by water, at some time or other, since the beginning of the eocene period. But, had I been then aware that a true pictorial representation of such modern revolutions in physical geography would have required the submergence of the Alps, Pyrenees, Apennines, and Carpathians, and the insertion of a few insignificant islands only in their place, I might have thought such an illustration superfluous or without meaning, and have been satisfied by simply insisting on the post-eocene ubiquity of the ocean—not indeed by a simultaneous, but by a successive occupancy of the whole ground. But how small a portion even of the superficial remodelling of the earth's crust in recent times is expressed, by declaring that we can establish by direct proof or legitimate inference the upheaval out of the sea of all the land in Europe, Asia, and part of Africa ! During the same tertiary periods there have been vertical subsidences as well as elevations of the same areas ; and we have every reason to believe that the larger part of the globe (comprising nearly three-fourths of its superficies), which is covered by water, has undergone, in equal periods of time, oscillations of level not inferior in degree to those to which the continental spaces have been subjected. If therefore we were to confine our thoughts to the mere outward modifications in the shape of the land or bed of the sea, and all the changes of climate and fluctuations in organic life inseparably connected with movements which have amounted, in some cases, to more than two miles vertically in one direction, besides the lateral displacement of rocks and their denudation by water, the series of events would seem endless, and their magnitude not easily to be exaggerated. But it is evident that these superficial mutations are trifling in amount in comparison with revolutions which must have been going on simultaneously in the inferior parts of the earth's crust. The reality of these changes is certain, although their nature may be obscure ; for we can rarely catch even a glimpse of the subterranean products of the eocene, miocene, and pliocene epochs, because it requires far more time than the tertiary periods have as yet furnished, to allow the disturbing causes to uplift, depress, and rend open, or for the ocean to denude the incumbent rocks so as to make it possible for an inhabitant of the surface to behold them and appreciate their magnitude.

The Alps indeed, where the convulsions have been greatest, reveal to us some monuments of the vast chemical changes and re-arrangement of the component elements of rocks which have taken place since the deposition of the eocene strata, and we thus gain some insight into the nature of the transformation of mineral masses which must have been going on contemporaneously at greater depths. It appears, for example, that in some places granite has been intruded into the axis of the Alpine chain, and that in other places various granitiform compounds have been formed since the whole nummulitic formation was elaborated beneath the sea. "In passing," says Sir R. Murchison, "from east to west, from the Austrian into the Savoy Alps, the zone of metamorphism widens laterally, from the centre to the flanks of the chain, so as to affect even the younger secondary deposits, and in one or more tracts even the tertiary, some of the strata called flysch being converted into a crystalline state.*" Instances are also adduced in the Bernese Alps (by the same author) of bands of granite or granitic schists in the midst of the flysch, demonstrating that the action of heat and vapours, or the causes commonly called plutonic, have changed even these modern deposits into gneiss, as well as into quartz rock and mica schist.†

To whatever geological period we may be disposed to assign the first origin or crystallization of the talcose granite and gneiss of Mont Blanc and other parts of the central nucleus of the Alps, we cannot doubt that they broke through the crust, and were protruded into the atmosphere, or were laid bare by denudation, after the nummulitic limestone was formed, and consequently after the beginning of the eocene period. For my own part, I have little doubt that these granites are all tertiary, and that they may even have passed from a fluid or semifluid state to their present form at an epoch more modern than the eocene period. But although it is only in a few narrow strips of country, like the Central Alps, that nature discloses to us some of the nether-formed rocks of such modern geological eras, we cannot doubt that still greater modifications of the interior have extended downwards for many miles or leagues in depth beneath the Alps, and beneath every region, whether of land or sea, which has risen, sunk, or oscillated in level since the fossil shells and zoophytes of the lower eocene period were living in the sea. The imagination of the geologist strives in vain to form a just conception of the extent of these internal modifications of the crust, of which we are only beginning to interpret the outward signs. How much fracture and dislocation of solid rock must have taken place! how much heating and cooling, expansion and contraction, drying and baking, softening and re-solidifying of sedimentary strata! Over how vast an area, and to how great a depth, often hundreds of yards or

* Quart. Journ. of Geol. Soc., vol. v. p. 164.

† Ibid. vol. v. p. 213.

several miles beneath the surface, have mineral masses been injected by lava, or dissolved by thermal waters, or corroded by acids, or permeated by steam, or impregnated with magnesia, sulphuric acid, or other substances introduced in a gaseous form! What obliteration has there not been of organic remains, and of the signs of stratification, in the course of the tertiary ages which have elapsed since the nummulitic strata and incumbent fucoid grits lay submerged beneath the ocean!

Sir Roderick Murchison has given a graphic description of the foldings, so sharp and so often repeated, of a grand succession of sedimentary strata in the Alps. Among other examples, he has cited one case of extraordinary inversion of large masses in the canton of Glarus, examined by himself and M. Escher, where a limestone of the Jurassic period containing ammonites is, on the one hand, "overlaid by a zone of talc and mica schist, having in parts quite the aspect of a primary rock;" while in another direction it is continuously superimposed for miles on beds of highly inclined flysch of eocene age.*

It seems that in the course of the stupendous movements which have raised these modern beds to the height of 8000 feet above the sea, and caused portions of them to become crystalline or metamorphic, large masses of the solid Jurassic limestones of the Oxfordian age have been pushed bodily out of their place, and planted unconformably on the edges of strata of the nummulitic series. Our indefatigable colleague naturally shrinks from offering any explanation of so marvellous and anomalous a state of things, extending as it does over a considerable area. In attempting to estimate such gigantic movements, the powers of imagination, he says, are at fault; and "surely," he adds, "it is not unphilosophical to believe that in those days the crust of the earth was affected by forces of infinitely greater intensity than those which now prevail." In particular, he regards the apparent inversion of the tertiary molasse along the flanks

* Quart. Journ. of Geol. Soc., vol. v. p. 246.

of the Alps, and its great elevation, as "a clear demonstration of a sudden operation or catastrophe."*

Now, I shall first venture to remark, in regard to these theoretical views, that the Alps, when considered as a mountain-chain which has originated entirely since the commencement of the tertiary period, bear emphatic and irrefragable testimony to the fact, that the intensity of the causes which have disturbed the crust of the globe has not diminished in the tertiary as compared to the secondary or primary fossiliferous epochs. It may possibly be still contended, that the energy and violence of the movements were more general in those earlier epochs, supposed by some to have been close upon the confines of "the reign of Chaos and Old Night;" but it cannot be pretended that there are any proofs of a more magnificent development of the disturbing forces in any given region of equal extent, and accomplished in an equal lapse of time, at any period antecedent to the upheaval of the Alps. If, however, any one should maintain, that in the earlier ages the movements which upheave, depress, and derange the position of strata were more general, and that they agitated simultaneously much wider horizontal areas, it will be easy to adduce the most overpowering evidence to the contrary. The wide extent in the United States of America, and in parts of Russia, of Carboniferous, Devonian, and Silurian strata, which, although upraised above the sea, continue almost as level as when the beds were first thrown down beneath its waters, clearly demonstrates the limitation of the agency to which great foldings and contortions of stratified rocks have been due to very confined spaces in each epoch. Were it otherwise, the multiplication of such extensive convulsions during a long succession of ages would have made it impossible to find any spot on the globe where the oldest rocks had escaped extreme derangement. It only remains therefore for the advocates of the paroxysmal hypothesis to assert that, although the disturbing forces have by no means grown feebler in the modern or tertiary times, as compared to periods when the oldest of the known strata

* Quart. Journ. of Geol. Soc., vol. v. p. 258.

were deposited, yet there have been brief eras of convulsion on a very grand scale, when the ordinary repose of nature was violently interrupted in particular regions (as in the Alps, for example) in a manner wholly different, in regard to the magnitude of the effects produced, from any which we have witnessed in historical times, or which ever occurred formerly during the ordinary and normal state of the globe.

That doctrines of this kind are popular, I am well aware; and if you desire to know how many modern writers have declared in their favour, I refer you to the excellent work which has just been published by one of our foreign members, M. d'Archiac, on the "The History of the Progress of Geology from the years 1834 to 1845." He has executed conscientiously nearly half of the laborious and delicate task assigned to him by the Geological Society of France, and has given us a faithful digest of memoirs written in a variety of languages and scattered through the Proceedings and Transactions of numerous scientific bodies, or the periodical magazines and journals of almost every civilized country. A geologist of practical experience in the field, as well as of extensive erudition, was required to make a good classification of such complex materials, and justly to appreciate their relative value. In M. d'Archiac's pages every author of merit has been allowed an impartial hearing, and the expositor's own occasional criticisms are not obtruded too prominently on the reader's attention; when they are offered, they are so judicious as to aid us materially in understanding the faithful analysis he has given of the opinions of others. In the concluding part of his chapter on "Le terrain moderne," and when speaking of active volcanoes, and in other places, he stoutly denies the adequacy of the causes which have modified the earth's crust in historical times to produce effects such as may enable us to explain geological monuments. "We must have recourse," he says, "to other causes, both organic and inorganic, of a more energetic and even paroxysmal character."*

On this subject I must make two preliminary remarks:

---

* Archiac, Hist. des Progres, &c. tome i. pp. 209, 670.

First, that our present inability to decipher some of the monuments of past ages by a key derived from the effects of causes now acting, ought never to be adduced as an argument of much weight in favour of the paroxysmal theory; for it might with equal or greater propriety be urged as a reason for believing in the adequacy of existing causes, or their identity with those of former times, since no one doubts that we are ignorant of the nature of many subterranean and suboceanic changes now in progress. If therefore there was nothing obscure or mysterious in geological phenomena, if they simply presented to us a picture of objects as familiar as the lavas of Vesuvius or the calcareous tufas of mineral springs, or the newly-formed deposits of a delta seen at low water, we should be entitled to suspect a great want of analogy between the ancient and modern processes at work above and below the earth's surface. We should then be entitled to ask, where are the nether-formed and deep-sea formations of the olden time? Where are the signs of those changes brought about in the bowels of the earth corresponding to such as are now in progress in regions inaccessible to human observation? Why have not the causes which have upheaved mountains and deeply fissured the rocks, or which have denuded large areas, revealed to us ancient stratified and unstratified rocks, wholly distinct from any which we now see generated by ordinary volcanic action or formed in lakes and shallow seas? Secondly, it should be thoroughly understood that the decision of the question at issue can in nowise be determined by simply comparing the magnitude of the changes brought about in historical times with those of antecedent periods. It may be safely affirmed, that the quantity of igneous and aqueous action,—of volcanic eruption and denudation,—of subterranean movement and sedimentary deposition,—not only of past ages, but of one geological epoch, or even the fraction of an epoch, has exceeded immeasurably all the fluctuations of the inorganic world which have been witnessed by man. But we have still to inquire whether the time to which each chapter or page or paragraph of the earth's autobiography refers, was not equally immense when contrasted with a brief era of 3000 or 5000 years. The real point on which the whole controversy turns, is the relative amount of work done by mechanical force in given quantities of time, past and present. Before we can determine the relative intensity of the force employed, we must have some fixed standard by which to measure the time expended in its development at two distinct periods. Dr Whewell has justly observed, that "mechanical power retains its amount, however much it be distributed through time and divested of the character of extraordinary violence,"*—a principle which should never be lost sight of when we contrast the effects of the historical with those of antecedent epochs. It is not the magnitude of the effects, however gigantic their proportions, which can inform us in the slightest degree whether the operation was sudden or gradual, insensible or paroxysmal. It must be shewn, that a slow process could never in any series of ages give rise to the same results.

The advocate of paroxysmal energy might assume an uniform and fixed rate of variation in times past and present for the animate world, that is to say, for the dying-out and coming-in of species, and then endeavour to prove that the changes of the inanimate world have not gone on in a corresponding ratio. But the adoption of such a standard of comparison would lead, I suspect, to a theory by no means favourable to the pristine intensity of natural causes. That the present state of the organic world is not stationary can, I think, be fairly inferred from the fact, that some species are known to have become extinct in the course even of the last three centuries, and that the exterminating causes always in activity, both on the land and in the waters, are very numerous; also, because man himself is an extremely modern creation; and we may therefore reasonably suppose that some of the mammalia now contemporary with man, as well as a variety of species of inferior classes, may have been recently introduced into the earth, to supply the places of plants and animals which have from time to time disappeared. But granting that some such secular variation in the zoological and botanical worlds is going on, and is by no means

* Quart. Journ. Geol. Soc., vol. iii. p. 231.

wholly inappreciable to the naturalist, still it is certainly far less manifest than the revolution always in progress in the inorganic world. Every year some volcanic eruptions take place, and a rude estimate might be made of the number of cubic feet of lava and scoriæ poured or cast out of various craters. The amount of mud and sand deposited in deltas, and the advance of new land upon the sea, or the annual retreat of wasting sea-cliffs, are changes the minimum amount of which might be roughly estimated. The quantity of land raised above or depressed below the level of the sea might also be computed, and the change arising from such movements in a century might be conjectured. Suppose the average rise of the land in some parts of Scandinavia be five feet in a hundred years, the present sea-coast might be uplifted 700 feet in fourteen thousand years; but we should have no reason to anticipate, from any zoological data hitherto acquired, that the molluscous fauna of the northern seas would in that lapse of years undergo any sensible amount of variation. If a botanist were asked how many earthquakes and volcanic eruptions might be expected, and how much the relative level of land and sea might be altered, or how far the principal deltas will encroach upon the ocean, or sea-cliffs recede from the present shores, before the species of European forest-trees die out, he would reply that such alterations in the inanimate world might be multiplied indefinitely before he should have reason to anticipate, by reference to any known data, that the existing species of trees in our forests would disappear and give place to others. In a word, the movement of the inorganic world is obvious and palpable, and might be likened to the minute-hand of a clock, the progress of which can be seen and heard, whereas the fluctuations of the living creation are nearly invisible, and resemble the motion of the hour-hand of a time-piece. It is only by watching it attentively for some time, and comparing its relative position after an interval, that we can prove the reality of its motion. If therefore in the coal-measures of South Wales or Nova Scotia we find the same fossil trees repeated through a mass of strata formed in shallow water 10,000 feet thick, we ought not to feel surprised, but merely conclude that formerly, as now, the rate of change in the vegetable kingdom was extremely slow, so that a stupendous mass of stratified sand and mud, as well as great revolutions in physical geography, might be slowly effected, without there being time for any important fluctuation to be brought about in the species of plants inhabiting the globe.

I have endeavoured to shew in my "Second Visit to the United States,"* that a great oscillation of level has taken place in the valley of the Mississippi and its tributaries, by means, first, of a slow downward movement, and then of an ascending one, and that the whole was accomplished since the period when the freshwater and land-shells now inhabiting that great valley were already in existence. We ought not therefore to be surprised when we discover sea-beaches in Norway 700 feet high, in which the shells are identical with those now inhabiting the German Ocean; for we have already seen that the rise of land in Scandinavia, however insensible to the inhabitants, is rapid when compared to the rate of contemporaneous change in the testaceous fauna. Were we to wait therefore until the mollusca shall have undergone as much fluctuation as they underwent between the period of the liassic and upper oolite formations, or still more between the oolite and chalk, or between the Wealden and eocene strata, what stupendous revolutions in physical geography ought we not to expect, and how many mountain-chains might not be produced by the repetition of shocks of moderate violence, or by movements not even perceptible by man! I may take this opportunity of stating, in reference to the permanent effects of subterranean movements in our times, that in all likelihood we are always in danger of underrating their intensity, because we can only measure their amount on the sea-coast, whereas the adjoining mountain-chains seem generally to be more shaken by earthquakes, and probably undergo a greater change of level than the low countries.

Let us now return to the Alps, and inquire whether geologists who ascribe their origin to paroxysmal forces have been

---

* Vol. ii. chap. xxxiv.

able of late years to bring to light any new facts in support of their favourite doctrine. On the contrary, if I mistake not, they have been more and more compelled to assign the time during which the disturbing power was exerted to a succession of distinct geological periods, in some of which the force must have operated very slowly, while in other cases where it was sudden it may probably have been intermittent, and consisted, as in ordinary volcanic action, of a repetition of shocks or explosions of moderate intensity. In illustration of these principles, I may first mention that some of the volcanic eruptions of the Alps, which produced the porphyry called melaphyre, broke out again and again, as M. Favre has demonstrated, in the sea of the Jurassic period, and they were accompanied and followed by metamorphic action, occasioned by gaseous emanations. The tuffs and trap dikes of Monte Bolca and the Vicentine show that other volcanic eruptions poured out lava and ejected scoriæ into the waters of the eocene sea. Again, after this period, the protrusion, if not the formation, of the talcose granite, or protogene of the central nucleus of the Alps, occurred. The upheaval of nearly the whole mountain mass, from the waters of the eocene sea to an elevation of more than two miles above its level, happened subsequently to the deposition of all the nummulitic beds and the flysch. These latter deposits, thousands of feet in thickness, shared, after the commencement of the tertiary period, in all the movements, whether slow or convulsive, to which the Alpine rocks owe their curvatures, dislocations, and vertical or lateral displacement. The grand sinking-down of the nagelflue or conglomerate of the molasse, more than a mile vertically, belongs again to a still later period, which did not begin till all the eocene movements had terminated, and was due to a gradual subsidence along the whole northern flank of the chain. At a still more modern era, the entire upheaval of the same molasse took place, so that it reached at length its present altitude of 3000 or 4000 feet above the sea. Nor did the uplifting agency cease here, for it continued till the newer or subapennine tertiary beds were made to emerge. There are proofs indeed of the relative level of sea and land having been modified even after the erratic blocks were conveyed to their present sites, or subsequently to the glacial period of Northern Europe.

This assignment to a great number of distinct and separate periods of the work done by the moving and disturbing powers, is by no means the result of the study of the Alps exclusively. In other mountain-ranges it is now ascertained that the upheaving and depressing forces have been propagated in succession along the same parallel zones of country; and M. Elie de Beaumont has frankly confessed that he was in error when he first pronounced the Pyrenees to be a chain due to a single upthrow, " un seul jet," or " une chaîne élevée en une seule fois." He and M. Dufrénoy now go so far as to agree with M. Durocher, that in the Pyrenean chain, in spite of the general unity and simplicity of its structure, six, if not seven systems of dislocation, each chronologically distinct from the other, can be made out.*

In regard to the Alps, it appears from the observations of Leopold Von Buch, Sir Roderick Murchison, and others, that whatever be the major axis of the crystalline mass in the centre, such also is the prevailing direction of all the sedimentary deposits which lie on either side of the chain. Whether the axis be composed of granite, syenite, gneiss, mica-schist, marble, dolomite, or of any rock formed by eruption or by the metamorphism of pre-existing strata, there is obviously some connection between the position of the central crystalline nucleus and the dominant strike of the flanking deposits. It is as if the intrusion of the igneous matter at certain periods had not only raised the chain, but so injected and distended its central parts, as to force outwards the pliant strata on each side, and to cause them to fold themselves into parallel anticlinal and synclinal flexures.

The theory first proposed by Von Buch, of the conversion of mountain masses in the Tyrol and other parts of the Alps into dolomite, and of other limestones into gypsum, has been gradually embraced by the majority of the most eminent geologists who have carefully examined the great chain. The

---

* Bulletin, 2d Series, vol. iv. p. 1368.

porous and cavernous nature of the dolomite are referred to by MM. É. de Beaumont and Morlot as a character implying the alteration of a compact rock into one of more open texture which had been permeated by gases.* "It is now more than twenty years," says De Beaumont, writing in 1847, "since I first advocated Leopold Von Buch's views, who attributed the gypsums and dolomites of the Alps to *épigénie*, or to the alterations of calcareous masses by mineral springs and gaseous emanations which came up from the interior of the earth at the time when the porphyries called melaphyre were formed.† M. Frapolli, in reference to similar metamorphic action, has adduced numerous facts illustrative of the manner in which carbonates of lime may have been turned by sulphurous vapours into gypsum; and Sir R. Murchison reminds us that the well-known thermal waters of Aix do now actually change the ordinary Jurassic limestone into sulphate of lime; while, according to M. Coquand, another example of the like metamorphism is afforded by Mofettes, where the sulphuro-hydrous emanations turn the cretaceous limestone into gypsum along the lines of fissure which they permeate.‡ M. Favre, as before stated, has shewn that the period when the porphyries called melaphyre were erupted agrees well with this hypothesis, and that the heat and gases disengaged during such volcanic outbursts might well have transformed the calcareous into magnesian rocks. Thus it is supposed that the carbonate of lime containing shells of the Jurassic epoch has been slowly transformed into magnesian carbonate, and perhaps an increase of volume was gradually acquired by the gypseous and dolomitic masses in proportion as they derived fresh accessions of mineral matter from below. If so it may have caused expansion, and have furnished an irresistible lateral pressure.

If in the central parts of the Alps we suppose heat to have accompanied the metamorphic action which has converted into gneiss and mica-schist, not only the Jurassic and creta-

* Bulletin, 2d Series, vol. vi. p. 318. † Ibid. vol. iv. p. 1282.
‡ Ibid. vol. vi. p. 124.

ceous, but even certain eocene strata, this same heat must have caused many kinds of rock to expand, and might, in this manner, slowly give rise to the sideway thrust exhibited in the curved beds on either flank of the chain. It is now known that granite and sandstone, while solid, expand and contract, even under such a range of atmospheric temperature as the difference of a Canadian winter and summer produce. We must also take into account that highly inclined or vertical argillaceous strata, such as the flysch, would shrink when heated, and give off their water; while other rocks, ranged side by side, might be simultaneously expanding or partially melting, so as to occupy more room, and that the clays might thus be pressed into solid shales and acquire irregular and complicated curves. The irregularity and confusion would be greatly increased by local variations in the composition of the stratified deposits, whether in the direction of their strike or dip, and also by the unequal intensity of the heating and cooling processes, whether the central be compared with the lateral parts of the chain, or the superficial with the internal parts. Yet we cannot feel sure, that were such mighty changes now in progress in any range of mountains subject to earthquakes, such as the Andes or Himalaya, we could guess at the direction of the movement, for the contraction or expansion of mineral masses might be carried on as slowly as the growth of a tree or the swelling of its roots in the soil.

M. de Beaumont, in his essay on volcanic and metalliferous emanations,* observes that, according to the experiments of Deville, the contraction of granite in passing from a melted or plastic to a solid state must be more than ten per cent. We have here then at our command an abundant source of depression on a grand scale at every geological period in which granitic rocks have originated. All mineralogists seem agreed that the passage from a liquid or pasty to a solid and crystalline state cannot, in such cases, have been instantaneous throughout voluminous masses; yet by suddenly crystallizing alone could it have given rise to the

* Bulletin de la Soc. Géol. 2d Series, vol. vi. p. 1312.

paroxysmal downthrow of overlying rocks. On the contrary, every hypothesis seems to proceed on the assumption that the crystallization of granite was an extremely gradual process. Many very instructive speculations on this head will be found in the writings of Scheerer, Frapolli, Fournet, Durocher, De Beaumont, and others, who have attempted to explain the reciprocal penetration of the crystals of quartz and felspar which enter into the composition of granites. These minerals, as is well known, have crystallized in an order independent of their relative fusibility, the quartz not only imprinting its form on the felspar, but sometimes itself receiving the imprint of the crystals of felspar. Gaudin and Fournet, in order to account for this fact, have shewn that dissolved flint may cool without solidifying, and remain in a gelatinous state, and thus crystallize after the felspar and mica; while M. de Beaumont has suggested that electric action may prolong the duration of the viscosity of silex.*

The conglomerate of the molasse called nagelflue, before alluded to, and referred to the miocene, if not in part at least to a still later (pliocene) date, attains in some places a truly wonderful thickness, exceeding 6000 and even 8000 feet. It is very conspicuous in the Rigi and in the neighbourhood of Lucerne, as well as in the Speer near Wesen. The lower part of the group, containing terrestrial plants, fluviatile shells, and the bones of extinct land-quadrupeds, is considered by M. Escher as a freshwater formation, while some of the sandstones and marls of the upper members of the series contain marine shells.† To explain the origin of such a succession of pebbly strata, we are naturally referred, by Studer, Escher, Sir R. Murchison, and others, to a long-continued depression along the whole external northern face of the Alps. Numerous torrents are supposed to have issued from the islands which then occupied the site of the loftiest portions of the chain, and the continuity of the strata is explained by imagining them to have accumulated on a shelving shore like that of the present maritime Alps. At first the materials

* Bulletin, 2d Series, vol. iv. p. 1022.

† Murchison, ibid. p. 229.

must have been arranged in beds which sloped away from their parent rocks of the Alps; yet after sinking successively to enormous depths, they have been brought up again, so as to dip towards the older rocks, as if they passed under them.

The first part of this grand subsidence of the sea-bottom was doubtless analogous to that now in progress on part of the coast of Greenland. But if the adjoining land participated in the same downward movement, it is difficult to conceive how it escaped being submerged, or how it could continue to retain its size and altitude so as to continue to be the source of such an inexhaustible supply of pebbles. We can scarcely avoid speculating on a contemporaneous slow upheaval of the mountains. There may have been an ascending movement in one region, and a descending one in a contiguous parallel zone of country, as the northern part of Scandinavia is now rising while the southern portion in Scania is sinking, or at least has sunk within the historical period. Perhaps the not uncommon occurrence, of deep sea in the immediate vicinity of bold coasts and mountain-chains, may be connected with extensive lines of fault, parallel to the shores, on the opposite sides of which vertical movements may be taking place in contrary directions, or one side may be motionless, while the other is subsiding. In no other way does it seem possible to account for the proximity, throughout a long series of ages, of high land, and of a sea-bottom always going down so gradually as to remain for a long time the receptacle of annual tributes of rolled pebbles, and acquiring in the end a thickness of 5000 and 8000 feet. In regard to faults which have shifted rocks several thousand feet in a vertical direction, it is often too hastily assumed that they must have been produced suddenly; whereas the reverse is indicated by the fact that the walls of such faults are rubbed, polished, and striated, as if they had been subjected to friction long continued, or many times repeated. The mass moreover of fragmentary matter usually included between the opposite walls of such rents is partly reduced to fine clay or dust, and partly filled with stone which have been superficially scored in various directions.

The minute study of the structure and organic contents of strata of various ages, has made us of late years more and more familiar with the hypothesis of a slow sinking of the ancient floor of the ocean going on while it was receiving repeated accessions of sediment. We must not forget that in all such cases a solid foundation of subjacent rock of unknown depth, and perhaps much older than the newly superimposed deposit, is undergoing simultaneously a change of position, and that rocks still lower are undergoing, whether by cooling or crystallizing, a change of structure. These very gradual movements are quite as remarkable in the palæozoic as in the tertiary periods. By consulting the "Memoirs of the Geological Survey of Great Britain," you will learn that in Wales, and the contiguous parts of England, a maximum thickness of 32,000 feet (more than six miles), of carboniferous, Devonian and Silurian beds, has been measured, the whole formed whilst the bed of the sea was continuously and tranquilly subsiding. In illustration of a movement of the same kind, I need scarcely remind you of the coal-measures of South Wales, with their numerous under-clays, each containing Stigmaria, a phenomenon to which Mr Logan first drew our attention. Mr Binney of Manchester has since proved to us that all these Stigmariæ, found in the floor of every coal-seam, are the roots *in situ* of fossil trees, chiefly of the genus *Sigillaria*, and that they are occasionally attached to their stems or trunks,—a conclusion fully confirmed by the more recent observations of Mr Richard Brown on the coal-fields of Nova Scotia. Sir Henry De la Beche also, in his paper on the rocks of South Wales and the south-west of England, confirms these statements, and shows that subsidences of vast amount took place slowly during the accumulation of the palæozoic strata, the sea all the while remaining shallow, in spite of a depression of one or two miles. Still later, Professor John Phillips, in the second volume of the same "Survey," has pointed out analogous phenomena in the old red sandstone of the Forest of Dean; and these strata, 7000 feet thick, are described as having been formed in a sea of moderate depth. Fossil corals and shells imbedded as they grew, or ripple-marked sandstones and sandy or gravelly strata with subordinate diagonal layers, confirm these views. Such movements took place contemporaneously with the growth of organic matter, just as subsidence on a grand scale is now going on over vast areas in the Pacific and Indian Oceans,—a class of facts on which Mr Darwin has founded his theory of atolls, or the origin of annular coral islands with lagoons. His theory, as you have probably observed, has been recently embraced and more fully elucidated by Mr Dana, in his valuable chapters on the geology of the American Exploring Expedition under Captain Wilkes.

The investigations of Professor Edward Forbes, on the laws governing the distribution of marine animal life, at various depths in the Mediterranean, have powerfully aided us in determining the conditions under which particular strata were formed, the depth of water being deducible from a careful study of the organic contents of each bed. Availing themselves of this key, Captain Ibbetson and Professor Forbes have shown how the lower cretaceous strata of the Isle of Wight have been deposited on a gradually sinking submarine bottom, while Mr Prestwich has applied the same method of reasoning, with equal success, to the eocene strata of Alum and Whitecliff Bays in the same island.* In this instance it is remarkable, that after a depression of 1800 feet very slowly effected, there was still contiguous land inhabited by the Palæothere of Binstead and Hordwell and its contemporaries, as well as a freshwater estuary, implying that the movements in different parts of that region were either very unequal or opposite, or that they consisted of great oscillations of level. It would be easy to cite a variety of continental authorities in support of the same principle, but enough has been said to entitle me to ask, whether the subsidence of mountainous masses, lying immediately beneath the floor of the ocean, brought about by such slow degrees, can possibly occur, without causing beneath many of the sunk areas, vast flexures of the strata, which as they sink for miles vertically must occasionally be forced to pack them-

---

* Prestwich, Quart. Journ. Geol. Soc. vol. ii. p. 223.

selves into smaller spaces than those which they previously occupied. If this be true, the contortions and foldings of pliant beds, and the fracture and dislocation of the more unyielding rocks, have frequently been due to movements as gradual as those of various ages to which I have been alluding.

The imagination may well recoil from the vain effort of conceiving a succession of years sufficiently vast to allow of the accomplishment of contortions and inversions of stratified masses like those of the higher Alps; but its powers are equally incapable of comprehending the time required for grinding down the pebbles of a conglomerate 8000 feet in thickness. In this case, however, there is no mode of evading the obvious conclusion, since every pebble tells its own tale. Stupendous as is the aggregate result, there is no escape from the necessity of assuming a lapse of time sufficiently enormous to allow of so tedious an operation. No intervention of a cataclysm or series of paroxysmal waves can avail us; and if the geologist could abridge the period, he would find that far from being a gainer, he had deprived himself of the only means ever yet suggested of explaining another set of geological monuments, relating to what we term denudation. It is not simply by fixed and permanent inequalities of level, in the land and sea, or by the alternation of dry and rainy seasons, or of summer heat and winter's frost, that the aqueous action of torrents, rivers, breakers, tides, and currents acquires a sustained energy, capable of denuding wide areas, but by the gradual elevation or subsidence of continents and islands, occasionally accompanied by many minor oscillations of level. It is by reiterated slight variations in the position of a coast line, by the continual shifting of the points of attack, that every portion of the surface of the land is exposed by turns to denudation, and is prevented from ever settling into a state of equilibrium and cessation from waste. If earthquakes agitate the country from time to time, while it is rising or sinking, so as to block up valleys and cause temporary lakes and fissures, or the fall of river-cliffs and sea-cliffs, the power of aqueous destruction will be still further augmented.

In the first volume of the "Memoirs of the Survey of Great Britain," Professor Ramsay has shewn that the missing beds, removed from the summit of the Mendips, must have been nearly a mile in thickness, and he has pointed out considerable areas in South Wales and some of the adjacent counties of England, where a series of palæozoic strata not less than 11,000 feet in thickness have been stripped off. All these materials have of course been transported to new regions; and when it is shewn by observations in the same "Survey" that the palæozoic strata are from 20,000 to 30,000 feet thick, we have a counterpart of older date of denuding operations on a scale of similar grandeur, for what has been carried away or borrowed from one space must always have been given to another. The gain must always have equalled the loss, and sediment deposited in one area must be the measure of the quantity of pre-existing rock cleared away elsewhere. The announcement of this principle may seem, perhaps, like insisting on a truism, but I find it necessary, because in many geological speculations I observe it is taken for granted that the external crust of the earth has been always growing thicker, in consequence of the accumulation of stratified rocks, as if they (and possibly the contemporaneous rocks of fusion, in progress far below) were not produced at the expense of pre-existing rocks, stratified and unstratified. Whether indeed the trap and granite of successive ages were formed by the melting of matter previously solidified, will be questioned by those who contend that the globe was originally a fused mass, and who also assume (still more gratuitously as appears to me), that geological monuments have reference to the period when the melted nucleus was passing to a more and more solid state. But even those geologists must admit that strata of the old red sandstone, or of any other ancient or modern rock of mechanical origin, imply the transportation from some other region, whether contiguous or remote, of an equal amount of solid material, so that the stony exterior of the planet has always grown thinner in one place whenever by accessions of new strata it has acquired density in another. The vacant space left by the missing rocks, after extensive denudation, may be less imposing to the imagination than a vast thickness of conglomerate or sandstone, or the bodily presence as it were of a mountain-chain with all its inclined and curved strata; but the denuded tracts speak a clear and emphatic language to our reason, and like mountain masses of fossil nummulites, or of corals and shells, or seams of coal based on under-clays full of Stigmaria and surmounted by erect fossil trees, demand countless ages for their origin, and these ages supply the time in which continents and mountain-chains may rise and sink, without sudden, instantaneous or paroxysmal action.

I have already alluded to the slow crystallization and consequent contraction of granitic mixtures, and to the expansion of solid rocks by heat, and to the melting of stony masses, together with various metamorphic agencies, as the causes of slow and gradual movement, both vertical and horizontal. Formerly, when the stratified materials of the Alps presented to the eye of every observer a confused heap of ruin, before any general laws governing the lines of longitudinal fracture, or the parallel foldings of the strata, were caught sight of, it might be argued, that such chaotic disorder implied one or more paroxysmal outbursts of subterranean force, wholly different from ordinary volcanic or any other known agency. But Sir Roderick Murchison agrees with an eminent foreign member of this Society, Professor H. D. Rogers of the United States, and with several Swiss geologists of distinction, that the dislocations and lateral movements of Alpine strata have been obviously regulated by general movements, in which system and law can be discovered. Mr Rogers, you will remember, declared in this room, when describing the structure of the Alps and Jura, that he recognized a striking analogy between the form of the flexures discernible in these European chains and those observed by him and his brother in the Appalachians of North America. In both cases the successive parallel folds have on one side a steep, short dip, while the other side of the anticlinal flexure is longer and less inclined. This longer side, in the Appalachians, or Alleghanies, dips towards the belt of intrusive volcanic rocks on the south-east flank of the chain. So in the Alps, the steep, short dips do not face the crystalline nucleus, but the longer and less inclined ones, except where a curve has been so great that the whole are made to dip one way, the more steeply inclined side having become, as it were, more than vertical.

In the Alps, the anticlinal folds, where they are greatest, dip inwardly towards the central peaks, and therefore in opposite directions on each flank of the chain. In the Jura, the steep, sharp dips of each parallel fold are upon the side, facing the Alps, and hence Professor Rogers imagines that the subterranean undulations in the earth's crust, which, according to his theory, gave rise to these flexures, were propagated, not from the Alps, but from the district of the Vosges, or the country towards the north-west. To this theory Professor A. Guyot strongly objects, arguing that it is more probable, on the contrary, that the immediate cause of the uplifting of the Jura is to be sought in the upheaval of the Alps. "The elevation," he remarks, "of the anticlinal ridges of the Jura diminishes gradually and regularly in proportion as the Jura recedes from the Alps, the summits sinking from 5000 to 2000 feet. The minor chains also of which the system of the Jura is composed are not exactly in the direction of the system itself, but oblique in such a manner as to be parallel with the chain of the Alps." There is in fact an intimate relation between the two chains, and M. Guyot conceives that the movement has been the result of a contraction of the terrestrial surface in consequence of gradual cooling, and that the folding has been due to lateral pressure resulting from this contraction.

It is not my purpose to enlarge at present on the rival theories thus brought forward to solve a most difficult problem; and I confess myself unable at present to understand how, according to the hypothesis of Mr Rogers, the grand flexures of the strata in mountain-chains can bear any intimate relation to great waves propagated through a subjacent reservoir of fluid matter. But if M. Guyot be correct in contending that a sinking-down of strata by gravity, owing to a slow contraction of part of the earth's crust below, can explain the flexures, we have then a cause introduced which might act as insensibly as the failure of support, so often witnessed in mines, especially after the removal of seams of coal. Such failure gives rise to what the miners call "creeps," which clearly prove that the sharpest bends and curvatures of yielding strata may be brought about by imperceptible degrees. Even if such an hypothesis be entitled, on pure mechanical principles, to equal favour, it should be preferred to one which appeals to extraordinary violence, for it must then be admitted, that the "*dignus vindice nodus*" has not yet occurred.

I have already suggested that the talcose or protogene granites of the Alps may belong to the tertiary period. M. de Beaumont believes that they were not protruded into the atmosphere till they had already reached the region of perpetual snow. Whether there may be good grounds for such an opinion or not, it does not appear to me to follow that such granites may not have been solidified at a considerable depth in the bowels of the earth. No sufficient reason seems to have been advanced to prove that they ought to be regarded, as the French geologist seems to infer, almost as superficial products.* The limestones, sandstones, and shales of the nummulitic and flysch series are of such enormous thickness, that tertiary granites may well be supposed to have crystallized beneath them, and then to have been exposed to view by breaking forth or bursting through the covering of sedimentary matter in the course of the enormous change of position which the Alpine eocene rocks have undergone. The question is one of the highest importance, because the French academician contends, that all the granites erupted in the earlier periods of the earth's history differed from those of later date, in being much more quartziferous; and he controverts the doctrine proposed by me in my "Elements of Geology," that the difference of mineral composition in the oldest rocks of this class now visible may reasonably and naturally be explained by imagining them to have originated at a great depth below the surface. On the contrary, M. de Beaumont supposes that granitic rocks charged with an excess of silice-

* Bulletin, 2d Series, vol. iv. p. 1299.

ous acid were formed at the surface in the older times, and he has even had the courage to present us with a diagram of Chaos, entitled " Chaos primitif," representing a scene by no means rude and disorderly, but where we behold two pyramidal mountains, from one of which the ordinary volcanic lavas and more volatile substances, such as sulphur, chlorine and aqueous vapour, are evolved ; while from the summit of the other, granitic compounds, tin, fluor, and the more refractory and less volatile materials, are discharged.* It is suggested that the greater part of the metals which usually accompany tin were concentrated in the first envelope of the globe, but after the palæozoic epoch they were withdrawn from circulation, and like the primitive granites ceased to be emitted from the interior. The gases and vapours, from which the more ancient metalliferous compounds were sublimed, would, it is said, have been most deleterious to organic beings living in the air and ocean, so that their evolution in the sea and atmosphere in later times was discontinued.

For my own part, after having given the most patient consideration to these views, I see no sufficient grounds for believing that the same granitiform mixtures and metalliferous emanations may not have been disengaged in equal quantity at every successive geological period down to the most modern. We are taught by the activity of several hundred volcanoes, that there must now be lakes and seas of melted matter in the interior of the earth, in every state, from one of perfect fusion to one of incipient crystallization ; and as solid rock must thus frequently originate in great masses, under conditions different from that of lava poured out into the atmosphere, why should we not adopt as the most probable conjecture the idea that this matter is now, as of old, passing into granite, or into some of the granitiform compounds, more especially when we know that silex abounds in many modern lavas, and that certain obsidians and pumice do not differ materially in their component elements from granite.

I fully assent to the doctrine so ably advocated by M. É.

* Bulletin, 2d Series, vol. iv. p. 1322.

de Beaumont, that a large class of metalliferous veins may simply be regarded as extinct mineral springs. They are fissures in which vapours, or thermal waters charged with various elementary bodies, have precipitated the materials of a refractory kind, or those which are the least easily retained in solution. The marked agreement between the contents of mineral springs and the emanations from active volcanoes strongly supports this view. But why should we doubt that fissures now existing in solid rocks may in like manner communicate at one extremity with subterranean masses of fused matter, while at their upper end they terminate in mineral springs? and if so, why may not hot steam and gases and mineral waters be depositing at this moment, as actively as ever, that class of elementary bodies, whether metalliferous or not, which we find in the oldest veins? The steam or hot water will always part with these substances in the deeper parts of every fissure, and merely bring up to the surface the residuary salts which are more soluble and volatile. Hence mineral veins are marked by the habitual absence of alkalies, which are so readily dissolved in water.

When we consider the grand and reiterated movements of elevation and depression which have agitated the earth's crust since the palæozoic epoch, and the vast amount of volcanic action which can be shown to have been of subsequent date, it is evident that all those refractory bodies said to have been "withdrawn from circulation," must have been from time to time re-melted, and therefore re-issued from the grand subterranean mint. Their circulation may always be confined to the interior of the earth, and they may never, except in very minute quantities, be disengaged superficially. If it be so, they must always be ancient in all future systems of geological classification; not because they originated at remote eras, but because time is required to uplift and expose them to view.

No illusion, indeed, is more likely to mislead us in our chronological speculations than the temptation to ascribe to antiquity appearances which are in reality characteristic of a deep subterranean or submarine origin. Volcanic rocks now forming at a certain distance below the surface, or sedimentary strata which are in progress in deep seas, can very rarely emerge and become visible to man till they have acquired a high antiquity relatively to most of the lavas and beds of mud, sand, and pebbles, which will be formed in the interval of time between the origin of such subterranean or submarine rocks and their exposure above ground. They cannot, except in a few very disturbed regions, like the Alps, emerge from the sea, or break out in the centre of a mountain-chain, till a series of grand revolutions of the earth's crust has occurred throughout many large areas. Lofty cones of lava and scoriæ will have been piled up, old rocks will have been denuded or displaced, bent or fractured, and new strata, thousands of feet thick, will have been formed, besides the occurrence of several important fluctuations in the organic world, before the nether-formed products of fire or water are brought into view. Whenever these do appear, their aspect will be strange and unfamiliar to human observers, such as might well belong to bodies formed in a part of the great laboratory of nature, to which man has no access. Such singularity in outward form and internal texture will naturally be referred to an origin connected with the beginning of things, if the mind be already prepossessed with a belief that we are studying the monuments of a planet, which has been passing from a chaotic or nascent state to one of order and maturity, especially if the peculiar rocks in question are found invariably to have claims to a high relative antiquity.

"Granitic eruptions," says M. de Beaumont, "have become more rare in the more recent epochs;" * and doubtless it is most true, that, in the newer secondary and older tertiary formations, the granitic rocks become more and more exceptional; but had we lived in the carboniferous or Permian epochs, we might, I conceive, with equal justice have declared the only granites then visible to be extremely ancient. The more quartziferous varieties, together with a certain class of metalliferous veins, posterior in date to the vegetation of the coal period, such as are now known to the miners of Cornwall, or to those of the Ural Mountains,

* Bulletin, 2d Series, vol. iv. p. 1299.

would then have been unformed, or at least invisible. The ages which have elapsed since the coal-measures were accumulated are so countless, as to have afforded ample time for the upheaval of much crystalline rock and metallic ores from great depths, and for the clearing away of superficial matter by aqueous denudation. To what an extent this subsequent denudation has been carried may be shown by adverting to the fact, that the masses removed must have more than equalled in volume all the sedimentary strata newer than the coal, for some part of the materials of such strata have been more than once ground down into sand or mud since that period and re-stratified.

Before concluding, I shall say a few words on another very different topic, yet one which has a distinct bearing on the theoretical question discussed in this Address. Until the transporting power of glaciers and icebergs was better understood, no geological phenomena were oftener appealed to in support of violent earthquake-waves, sudden deluges, rapid and overwhelming currents of mud, and other extraordinary agencies, than the northern and Alpine erratics scattered over hill and dale, and having no obvious relation in their geographical distribution to the present drainage or physical outline of the countries where they abound. The hypothesis which has recently gained more and more favour, as best explaining the dispersion of such blocks, dispenses with all sudden and paroxysmal exertion of force; nay, more, it does not even call into play a succession of waves such as ordinary earthquakes can produce. The rate at which huge blocks of stone travel for centuries on the surface of a glacier, never halting day or night, summer or winter, appears rarely to exceed, according to the exact measurement of Professor James Forbes, half an inch per hour. When the icy mass, with its moraine and included boulders, reaches the sea, and becoming detached on the coast, gives birth to an iceberg, the frozen raft traverses wide spaces of the ocean at the rate of a few miles a day, so that its advance is usually inappreciable by human sight. I have seen hundreds of these floating bergs at once in the Atlantic on their way southwards; but no observer could determine their direction, or decide whether they were aground or in motion, unless he had opportunities of comparing their relative position from day to day. So large is the volume of ice submerged beneath the water, that the waves and swell of the Atlantic during a storm have no more power to communicate a rocking motion to one of them than if they were islands, or parts of the firm land.

Should geologists ever be convinced that some of the most gigantic curvatures of Alpine strata have been the result of intense pressure, so moderated in its application as to have been just sufficient to overcome the resistance opposed to it,—should any of them ever declare their belief that the motion had been as insensible as the unfolding of the petals of a flower,—it would not imply a more remarkable revolution in popular opinion than we have witnessed in reference to the glacial hypothesis. Nor even then might we be entitled to pronounce the process a slow one relatively to other natural operations, organic and inorganic, which were simultaneously in progress. In the fourth volume of our Quarterly Journal (p. 70), Mr Hopkins, to whom you have this year awarded the Wollaston Medal, has published an excellent paper on the elevation and denudation of the Lake district of Cumberland and Westmoreland. He has undertaken, and, as it appears to me, with no small success, the very difficult task of restoring, in a series of diagrams, the successive steps by which the physical geography of the country attained its present condition, although the changes to be accounted for, consisting of the addition of several new sedimentary formations, and repeated alterations of level, and denudation of rocks, were numerous and complicated. In one part of his memoir he has suggested the possibility of the period during which the dispersion of erratic blocks took place, having extended far back in geological time, even as far as the oolitic period; an opinion which is, I think, at variance with a great weight of evidence derived from the study of the boulder formation both in Europe and North America. But in regard to the mode of transport, Mr Hopkins has taught us, that if the bed of the sea were suddenly uplifted from 100 to 200 feet in vertical height, such an instantaneous upward movement would give rise to currents having a velocity of twenty-five to thirty miles an hour, and these currents might move blocks of great magnitude from place to place. Thus a current of ten miles an hour would be capable of propelling a block of five tons weight, and its force increasing in the ratio of the square of its velocity, a current of twenty miles an hour would move a block of 320 tons. The experiments of Mr Scott Russell on the velocity of waves of translation, although made with much smaller waves, are supposed to bear out these views.*

Now, adopting all the mathematical and hydrostatical calculations of Mr Hopkins as correct, they prove, I think, the non-occurrence or extreme rarity in past times of earthquake-shocks more violent than such as we have experienced in the last ten centuries. For when we consider how many marine formations have been upraised, some of them from seas of considerable depth, and what a vast amount of upheaval and subsidence, estimated, as I have already reminded you, by miles vertically, has taken place, it seems clear that if currents and waves of such power as those contemplated by Mr Hopkins had really been set in motion, there would have been erratic blocks in deposits of all ages, instead of their being confined to the close of the tertiary period. Had these mighty waves swept again and again over the floor of the ocean, and over the land in ancient periods, a drift or boulder clay with rounded and angular blocks would have been conspicuous in the Eocene, Cretaceous, Jurassic, Triassic, Permian, Carboniferous, Devonian and Silurian formations, and would have been most strikingly displayed in such of these epochs as have been of the longest duration. I have seen fragments of gneiss eight feet in diameter in the base of the Silurian series in Canada, in the group called by the New York geologists the Potsdam sandstone;† but I observed in the same place similar gneiss *in situ*, in the immediate vicinity, so that the blocks may have been detached from an undermined cliff of the Silurian sea-coast. In like manner, in the valley of the Bormida, in Piedmont, there are huge rounded masses of serpentine in the tertiary molasse; but similar rocks *in situ* pre-existed in the same region, so that blocks may have been derived from the destruction of

* Hopkins, Quart. Journ. Geol. Soc. of Lond. vol. iv. p. 70, No. 13.

† See Travels in North America, vol. ii. p. 126.

cliffs close at hand. In Scotland, also, we see occasional fragments of large dimensions in the conglomerates of the old red sandstone, especially on the western coast, but in that case there is no ground for presuming distant transport. In no part of the geological series, except in that of very modern date, do we find an extensive deposit of drift, like that spread over Northern Europe and North America.

It may doubtless be objected, that by adopting the glacial hypothesis we concede the possibility of one natural agent, such as frost, acquiring at certain periods an intensity of action far greater than at others, and hence I may be asked, whether the energy of any other cause may not in an equal degree be subject to secular variation? I admit the force of the argument, if not pushed beyond its legitimate bounds. No one can contemplate future changes in physical geography without foreseeing that the varying altitude and extent of polar and equatorial lands may give rise to an intensity of solar heat or glacial cold, such as is not experienced now, and may never have been experienced on the earth; for the combinations of circumstances on which the climate of the globe most depend are so varied, that no one can define or guess how far heat, cold, moisture, and other conditions, may deviate from a mean state of things in the course of ages. But speculations of this kind belong equally to the future, the past and the present, and imply no inconstancy in the general condition of our planet, such as is assumed in the hypothesis of its passage from a chaotic to a fixed, stable, and perfect state. Living as we do in an era which has immediately followed the glacial epoch, we are able to comprehend the state of the northern hemisphere in European latitudes, when cold like that of the arctic and antarctic circles extended further from the poles towards the equator. We may also reason philosophically on the state of the globe during the carboniferous epoch, when there may have been little or no ice even at the poles. We may conclude that in those days a warmer, damper, and more uniform climate prevailed, when the Sigillaria, Lepidodendron, Caulopteris, Calamite, and other fossil plants flourished, and when there were reefs of coral in the adjoining seas. Such organic remains may betoken, as our Foreign Secretary, Mr

Bunbury, has argued, rather the absence of frost than, as many botanists once thought, an intense tropical heat. M. Adolphe Brongniart, in his admirable Essay on the genera of Fossil Plants, published in the year 1849,* has questioned, and apparently with reason, the proofs hitherto adduced in favour of the existence of any true palms in the coal-measures, and Mr Bunbury considers their absence as affording an additional argument to that derived from the universal preponderance of ferns in favour of a mild temperature in the atmosphere,—a warm, moist and uniform climate, not a tropical one. The flora, he says, of the London clay was of a much more tropical character. In this manner we may now reason philosophically on the remote carboniferous era according to strict rules of induction; but had we lived in that era, and had been called upon to decipher the monuments of a glacial period of high relative antiquity,—had the phenomena of the drift constituted the first or oldest chapter then extant of the earth's autobiography, instead of happening to be, as it now is, the last and newest, we should have been in danger of indulging for ever in the most visionary and extravagant hypotheses. Ignorant of glaciers and icebergs, and perhaps of ice and snow,—unable to comprehend the nature of that mysterious power which had polished the surface of rocks over wide areas, or had engraved upon them long rectilinear and parallel furrows, we should have gazed upon these markings, and upon the confused and unstratified heaps of clay and loam, interspersed with boulders, and usually devoid of fossils, in stupid amazement and with feelings of despair. The enormous bulk of some erratics, which had travelled for hundreds of miles from their original sites, would have confounded us, and might well have tempted a geologist to dream of frightful catastrophes, and diluvial waves of prodigious velocity, which swept over the planet in its infancy, before it was fitted for the reception of the higher animals and plants, much less to become the home of man. If any one then doubted that there had been an era of paroxysmal violence, or of primeval chaos, and wished to refer all geo-

* Tableau des genres, etc. Dictionnaire Universelle d'Histoire Nat., Art. Végétaux Fossiles.

logical appearances exclusively to the agency of slow and ordinary causes, he would have been asked to explain the position of fragments of granite, like those of Scandinavian origin, on the plains of Pomerania, or of protogene from Mont Blanc lodged on the summit of the Jura, and such an appeal in refutation of a theory apparently so visionary must have been triumphant.

But it is now time to conclude; and in taking leave of you, Gentlemen, I will venture to indulge the hope, that on some future occasion I may resume this theoretical discussion, which ought to embrace every department of geological inquiry, including that of palæontology, to which as yet I have been able to make but a few passing allusions.

Remarks on Three Naloo Negro Skulls. By RICHARD CULL, Fellow of the Ethnological Society.*

It is with considerable diffidence that I venture, at the request of our most zealous secretary, Dr King, to offer some remarks on the three Naloo negro skulls lying on the table.

The complete ossification of the cranial bones, the partial obliteration of the sutures, the ossific junction of the styloid process to the temporal bone, and the full development of the teeth, concur to indicate the mature age of the individuals to whom the skulls belonged. But the unworn condition of the teeth forbids us to infer that they were aged.

The skulls are marked A, B, and C, respectively. Those marked A and B are the skulls of men. The smaller malar bones, the more slender zygomata, together with the lesser development of those processes to which the powerful muscles are attached, would lead to the inference that the skull marked C is that of a woman, but Mr Whitfield knows it to be that of a man who fell in battle. The jaws project forward, especially in A, and least so in C. The malar bones do not appear to be laterally protuberant, but the zygomata are very much so, especially in A and C, where they are very convex.

In all three crania the sutures are less complex and more obliterated than in Europeans, especially in A and C. In all

* Read before the Society, 25th April 1849.

three the lamdoidal suture is most complex, and in that suture are several triquetral bones, but they are found near the junction with the mastoid suture, and not, as is most common, at the union of the sagittal with the lamdoidal. Triquetral bones are thought to be rare in African crania, and Blumenbach doubts if they are found in the crania of any savage races. These small bones, however, are as common in African as in European crania; and they are also as common in the crania of all savage races that I have examined.

There are modifications both of the forms and junctions of the several cranial bones. Thus, the sphenoid sometimes does not abut upon the parietal bone, but this modification is of rare occurrence in human crania. In all three Naloo skulls the sphenoid well abuts upon the parietal bone.

I shall not occupy the time by describing those modifications, because they are not constant; and, even if they were constant, they would not justify the adoption of the degrading term *inferior* to the negro race. The term inferior, applied to a race of men, is understood to mean inferior in those intellectual, moral, and religious qualities of mind which give dignity and goodness to character. Before we declare a race to be inferior in that respect, by the presence of any *physical* peculiarities, I think we ought to be satisfied that the inferiority depends on, and is a consequence of those physical peculiarities.

The character of a race depends upon its mental manifestations, and those depend on the brain. We have all of us witnessed Europeans with prominent jaws maintaining their position in civilized life. We have lately ourselves witnessed, in this Society, the talent of the Rev. Mr Hanson. Who is there that has not read of the virtues of Eustache? And there are ample records of many prognathous negroes who have distinguished themselves, and become brilliant examples of intellectual, moral, and religious greatness. But it is not merely a few superior individuals distinguishing themselves from the mass of the negro population,—there is now a large settlement, where civilization is making rapid progress, not by the substitution of a civilized race for that uncivilized one which is fast melting away, but by the civilization of the prognathous negro population which is now taking place in Liberia.

The physical man given to find the mental one, has been a problem which has engaged much attention. Physiologists have all rightly considered the brain as the organ of the mind. The question then arose, what are the peculiarities of brain on which the recognised peculiarities of mind depend? Camper invented his facial angle to measure the facial line, on the assumption that that line measures those peculiarities of brain. Blumenbach, Cuvier, Majendie, Tiedemann, Pritchard, and others, declare its failure; and Professor Owen correctly remarks, that the difference of the facial angle in the Naloo skulls depends on the different development of the incisive alveoli; and hence the facial angle does not even measure the relative prominence of the jaws.

Several other methods have been proposed to measure those peculiarities of brain, and I shall briefly refer to them, because they are important in relation to these Naloo crania, and will be necessary to my subsequent contributions on the craniography of the races of man.

Daubenton's angle is never heard of now. Blumenbach's observations fully proved its insufficiency.

Pritchard's method is condemned by Professor Owen, in his observations on these Naloo crania.

Daubenton long ago pointed out the relative situation of the foramen magnum in the head of man and of animals. It is placed much farther backwards than in the human head. It may be remarked, that this proposition is merely asserting in another form of expression, that the jaw of animals is more prominent than that of man. Professor Owen's objection to Camper's facial angle is equally fatal to Daubenton's proposition concerning the situation of the foramen magnum; and it is also equally fatal to those inferences which Dr Pritchard drew from Mr Owen's statements on the subject, in his memoir on the osteology of the Chimpanzee. And Dr Pritchard subsequently admitted, that the relative situation of the foramen magnum in human crania depends on the prominence of the alveolar process. There is no fixed ratio between the size of the human cranium and face; and there is no relationship between the amount of talent and the size of the face, as Sœmmering thought there exists.

"The brain," says Professor Tiedemann, in his paper on the brain of the negro, "is undoubtedly the organ of the mind. It is the part of our body which gives us the consciousness of our own existence, and through which we receive the impressions made upon the external senses, conducted to the brain by the nerves. Here the perceptions are compared and combined, so as to produce ideas. In this organ we think, reason, desire, and will. In short, the brain is the instrument by which all the operations called intellectual are carried on."* The chief value of the study of human crania is, that the cranium is a faithful record of the volume of the brain which was contained within it. Tiedemann determined the relative magnitudes of the Negro and European brains by weighing the quantity of dry millet seed with which he filled the crania. And Majendie says, "The only way of estimating the volume of brain in a living person is to measure the dimensions of the skull."†

Amongst other conclusions which Tiedemann draws from his observations is this:—"5. There is undoubtedly a very close connection between the absolute size of the brain and the intellectual powers and functions of the mind. This is evident from the remarkable smallness of the brain in cases of congenital idiotismus, few much exceeding in weight the brain of a new-born child. Gall, Spurzheim, Haslam, Esquirol, and others, have already observed this, which is also confirmed by my own researches. The brain of very talented men is remarkable, on the other hand, for its size."‡

I proceed to give some measurements of the Naloo crania, and to compare them with similar measurements of two Congo negro cranias, an Ashantee negro crania, and some European crania. The Naloo tribe is located on the banks of the Nunez, to the south of the Gambia, and in latitude 12° north. The Ashantee country extends northward to about 10° north latitude; and Congo is situated in 7° south latitude.

* Phil. Trans. 1836, p. 520.

† Majendie's Physiology by Milligan, p. 104.

‡ Phil. Trans. 1836, p. 502.

Diameter measurements made by Callipers.

The Naloo Crania.	A.	B.	C.
The longest diameter from the root of the nose to the protuberance just above the occipital spine, ...	$7\frac{3}{8}$	$7\frac{1}{2}$	$7\frac{1}{4}$
The transverse diameter from one meatus externus to the other, . . .	$4\frac{3}{8}$	$4\frac{5}{8}$	$4\frac{3}{8}$
Peripheral Measurements made by Tape.			
The greatest circumference of head taken over the root of the nose to the protuberance above the occipital spine, . .	$20\frac{1}{2}$	21	20
From the occipital spine over the head to the root of the nose, ...	$12\frac{1}{2}$	$13\frac{1}{4}$	$12\frac{1}{2}$
From the meatus externus over the head to the other meatus externus, . . .	$12\frac{1}{8}$	$12\frac{3}{4}$	$12\frac{1}{8}$

Subjoined are similar measurements of an Ashantee and two Congo crania in the Museum of the late M. Deville, and marked Nos. 1440 and 1668 in his collection. They are all three crania of adult, but not aged men. In all the jaws are prominent. For the convenience of reference, the Congo are numbered 1 and 2 respectively.

Diameter Measurements made by Callipers.	Ashantee.	Congo 1.	Congo 2.
The longest diameter from the root of the nose to the protuberance just above the occipital spine, . .	7	$6\frac{7}{8}$	$6\frac{7}{8}$
The transverse diameter from one meatus externus to the other, .	$4\frac{1}{2}$	$4\frac{1}{2}$	$4\frac{1}{2}$
Peripheral Measurements made by Tape.			
The greatest circumference of head taken over the root of the nose to the protuberance above the occipital spine, ...	$19\frac{1}{2}$	$19\frac{3}{8}$	$19\frac{1}{2}$
From the occipital spine over the head to the root of the nose, .	$12\frac{3}{8}$	$11\frac{1}{2}$	$12\frac{1}{2}$
From the meatus externus over the head to the other meatus externus,	12	$11\frac{1}{2}$	$12\frac{1}{4}$

The Naloo crania are larger than the Ashantee and Congo crania. And the Naloo marked B is larger than A and C.

It need scarcely be remarked that a much greater number of crania must be measured in order to obtain an average before we are justified in asserting that the Naloo negroes have larger brains than the Congo and Ashantee negroes.

TABLE of Measurements of European crania of men, comprising 3 *English crania of unknown persons;* 1 *also, Julien Hibbert, an English gentleman of fortune;* 2 *Highlanders of Scotland, viz., King Robert Bruce and the poet Burns; and* 1 *German, viz., Dr Spurzheim.*

DIAMETER MEASUREMENTS MADE BY CALLIPERS.	ENGLISH.				HIGHLAND.		GERMAN.
	A.	B.	C.	Hibbert.	Bruce.	Burns.	Spurzheim.
The longest diameter from the root of the nose to the protuberance just above the occipital spine, .	$7\frac{5}{8}$	$7\frac{3}{8}$	$7\frac{5}{8}$	$7\frac{1}{8}$	8	8	$7\frac{3}{4}$
The transverse diameter from one meatus externus to the other, .	5	$4\frac{3}{4}$	5	$4\frac{3}{4}$	6	$5\frac{3}{4}$	6
PERIPHERAL MEASUREMENTS MADE BY TAPE.							
The greatest circumference of head taken above the nose to the protuberance above the occipital spine,	$21\frac{1}{2}$	$20\frac{3}{4}$	$21\frac{1}{2}$	$20\frac{1}{4}$	$22\frac{1}{2}$	$22\frac{1}{4}$	$22\frac{1}{4}$
From the occipital spine over the head to the root of the nose, .	$13\frac{1}{8}$	$13\frac{1}{4}$	$13\frac{1}{4}$	$12\frac{1}{2}$	$13\frac{1}{2}$	14	$13\frac{1}{2}$
From the meatus externus over the head to the other meatus externus, ...	$13\frac{1}{8}$	$12\frac{3}{4}$	$12\frac{1}{4}$	$12\frac{3}{8}$	14	13	$13\frac{3}{4}$

The measurements of the six negro crania exhibit a less capacity for the brain than is found in European crania. It may be objected that these measurements will not enable us to ascertain with exactness the solid contents of the cranium, and that therefore the value of our comparison will be diminished. The measurements are not intended for that purpose, but they are sufficient to enable us to compare the sizes of crania. The smaller measurements of the negro crania are in harmony with Tiedemann's researches. He gives tables of the respective weights of millet seed, which was required to fill the cavity in 38 negroes, and in 77 Europeans. I have taken an average of his tables which is subjoined.

The average of 38 negroes, is 37 ounces, 6 drams, 18 grains.

The average of 77 Europeans, is 41 ounces, 2 drams, 30 grains.

Thus, the brain of the negro is considerably smaller than the European. And those of the Naloo tribe are no exception to other negroes, if we may suppose those three crania to be average specimens.

It is difficult to describe irregular solid forms with precision. The verbal descriptions of the various forms of national crania are unsatisfactory. The terms long and broad are vague, and we have gained nothing either in precision or convenience, by exchanging those terms for the cumbersome Greek compounds, brachycephalous and dolichocephalous. The term elongated, besides its vagueness, is suggestive of an artificial change of form, as are also the terms depressed and contracted. Mr Lawrence sums up his description of the Ethiopian variety of cranium thus, "Narrow and depressed forehead; the entire cranium contracted anteriorly; the cavity smaller, both in its circumference and transverse measurements.*

I proceed to add such measurements of the Naloo crania as will, with the preceding ones, exhibit their forms, their relative magnitudes, and the relative magnitudes of the regions in each cranium. The situation of the sutures in relation to the subjacent brain, is not sufficiently constant to serve as fixed points for our measurements. The situation of the tuberosities of the parietal and occipital bones are also inconstant. And therefore, we must select some other points. The phrenologists have supplied us with such points in their localization of the mental faculties, and whether we adopt or reject their physiology, we may avail ourselves of their description of the surface of the cranium as fixed stations for our survey. My friend Dr Browne, at my request,

* Lawrence's Lectures on Man, section 2, chap. 4.

has most kindly measured the Naloo crania from those points :—

PERIPHERAL MEASUREMENTS MADE BY TAPE.	A.	B.	C.
The greatest circumference of the crania measured from the organ named Philoprogenitiveness to the root of the nose,	$20\frac{1}{2}$	21	20
Circumference from the organ named Philoprogenitiveness to that named Comparison,	$19\frac{3}{4}$	$20\frac{1}{8}$	20
From the occipital spine over the head to the root of the nose,	$12\frac{1}{2}$	$13\frac{1}{4}$	$12\frac{1}{2}$
From the meatus externus vertically over the head to the opposite meatus,	$12\frac{1}{8}$	$12\frac{3}{4}$	$12\frac{1}{8}$
DIAMETER MEASUREMENTS MADE BY CALLIPERS.			
The long diameter is from Philoprogenitiveness to Individuality,	$7\frac{3}{8}$	$7\frac{1}{2}$	$7\frac{1}{4}$
TRANSVERSE DIAMETERS.			
Constructiveness to Constructiveness,	$4\frac{1}{4}$	$4\frac{1}{8}$	$4\frac{1}{8}$
Meatus to Meatus,	$4\frac{3}{8}$	$4\frac{5}{8}$	$4\frac{3}{4}$
Mastoid process to Mastoid process,	$5\frac{1}{10}$	$4\frac{9}{10}$	$4\frac{9}{10}$
Destructiveness to Destructiveness,	$5\frac{3}{10}$	$5\frac{4}{10}$	$5\frac{3}{10}$
Secretiveness to Secretiveness,	5	$5\frac{1}{2}$	$5\frac{3}{8}$
Cautiousness to Cautiousness,	$4\frac{6}{10}$	$5\frac{4}{10}$	$5\frac{3}{10}$
Ideality to Ideality,	$4\frac{2}{10}$	$4\frac{7}{8}$	$4\frac{1}{2}$

Measurements of radii from the meatus externus to various points on the mesial line which extends from the root of the nose to the occipital spine, and which is a peripheral line.

Meatus externus to Individuality,	$4\frac{5}{8}$	$4\frac{7}{10}$	$4\frac{3}{10}$
... ... Comparison,	5	5	$4\frac{7}{10}$
... ... Benevolence,	$5\frac{1}{8}$	$5\frac{1}{8}$	$4\frac{8}{10}$
... ... Veneration,	$5\frac{1}{8}$	$5\frac{1}{8}$	$4\frac{8}{10}$
... ... Firmness,	$5\frac{1}{8}$	$5\frac{1}{8}$	$4\frac{8}{10}$
... ... Self-Esteem,	$5\frac{1}{8}$	5	$4\frac{8}{10}$
... ... Inhabitiveness,	5	$4\frac{8}{10}$	$4\frac{7}{10}$
... ... Philoprogenitiveness,	$4\frac{1}{2}$	$4\frac{4}{10}$	$4\frac{1}{8}$

Thus in all three crania the basilar and posterior regions predominate over the coronal and the anterior regions.

On Prismatic Colours in Dew Drops. By the Rev. W. Scoresby, D.D., F.R.SS. Lond. and Edin., and Member of the Institute of France, &c. Communicated by the Author.

In a communication which appeared in the New Edinburgh Philosophical Journal for 1841, vol. xxxi., p. 50, I described a new method of observing the prismatic colours in the drops of dew deposited upon grass or other herbage, and the exquisitely rich and varied colours observed when the drops, receiving the rays of clear sunshine, are examined, in proper aspect, by means of a pocket telescope.

Since the period of my former communication, I have been in the habit, incidentally, from time to time, of repeating the observations in this interesting department of optics, whenever the circumstances were of a nature to afford any striking varieties in the phenomena. Two very marked and interesting varieties have since been observed, the description of which, I trust, will not be deemed unfitting as a supplement to my former paper.

1. The first of these varieties, now to be noticed, consisted in the beauty and diversity of the colours exhibited by minute, but regularly and thickly distributed, globules of dew on the surface of the leaves of plants horizontally spread. These were first observed in a morning in September, some years ago, and about two hours after sunrise, when a fog lay thick in a valley below my residence; but clear bright sunshine prevailed on the elevated ground. After a warm day, the night had been unusually cold, with a clear sky, and in the morning a very thick dew was found to be spread over the herbage and on the ground. I sought for some time for some additional phenomenon in this species of optical beauty, and examined, without success, a good many plants with leaves spread out flat on the ground, and thickly overspread with minute drops of dew. But, after many unsuccessful efforts, perceiving that, when *looking down* upon the horizontally spread leaves of the columbine, there was a strong glossy appearance and reflection of light, I applied my telescope to the object, but it was too near even for its short focal

distance. I then procured a table, and by placing an ottoman on it, I found, when standing on the summit, that I was now high enough to view the leaves distinctly. Looking down at an angle of from 60° to 70°, from a horizontal plane, I saw distinctly the whole surface of a leaf thickly bespangled as with the most exquisitely coloured gems of varied colours,—every colour of the rainbow being seen at the same view, though in somewhat irregular disposition, on the same leaf and at the same moment, and whilst, by a slight movement of the head, when still observing the appearances, the colours were made to shift and change as by a magical illusion. Scarcely anything in light and colour could exceed the brilliant variety and beautiful play of the colours as thus viewed from a small elevation.

On some leaves I found a disposition towards the prismatic arrangement in the various colours; and, ultimately, after several trials, I found a small row of very globular drops on the horizontal part of a leaf, presenting, in regular succession, this series of colours,—pale blue, sulphur yellow, orange red. On another leaf, an extended deposit of minute dew drops exhibited a like variety and arrangement, in which all the colours of the spectrum were exhibited, and very nearly in their proper order. The deviations from the proper order are clearly ascribable to defect in perfect sphericity.

In the former paper, the circumstances under which the dew was deposited—being in excessive profusion, probably, so as to overrun the surface on which deposited—occasioned the phenomena to be best observed in drops *pendant* from minute blades of grass; but here the drops lay, as most usually is the case, deposited chiefly on the upper surfaces of the grass and plants.

2. The second variety of these phenomena, which I have here to describe, consisted in the exhibition of prismatic colours in the minute globules of dew deposited on gossamer.

The colours, as observed in the dew on the gossamer web, I have found to be far more splendid, in respect to the immense aggregation of brilliant coloured points, than in any other form in which the phenomena had been observed.

In cases where the gossamer forms a pretty close and regular tissue, and is evenly stretched and rather broadly spread; and where the dew is profusely deposited on the different fibres of the web, and illuminated by a perfectly clear morning sun, we have the best conditions for the examination of the optical phenomena. For the purpose of the examination, a pocket telescope is the most convenient instrument; but it requires to draw out so far *beyond* the ordinary focus, as to afford a clear view of *near* objects, even as near as 6 or 8 feet to the eye. A convenient size of telescope is such as what I commonly use, a three-draw glass of one-inch aperture, and 12 or 13 inches focal length, drawing out nearly an inch and a half beyond the focal position for distant objects.

With the back of the observer towards the sun, and whilst the sun is not yet too high in the sky, certain positions will be observed, in which the dew-covered gossamer presents a surface of reflected light, and when, moving the head, slightly, right or left, a tinge of colour, more or less definite, will probably be seen. If the distance of the tissue from the eye be not less than the shortest range of clear vision in the telescope, the colours may be then examined and analyzed by the instrument; and, by slight changes in the place of the observer's head, made to pass through the whole of the prismatic variety. If the distance be too short for the telescope, by reason of the sun's high position in the heavens, that distance may be increased, after the manner already described, by interposition of a table, chair, or other piece of domestic furniture, betwixt the feet of the observer and the ground.

Observing in one or other of these methods, I have been quite charmed with the splendour of appearance of the dew-bespangled gossamer. Sometimes, where the breadth of the web was considerable, most of the colours of the spectrum have been observed on the same surface, in regular and beautiful series; and these colours made to shift or vanish, as in some optical experiments with polarizing instruments, with surprising effects in beauty.

In cases where the spider's lines are not very compact, as in some observations recently made at Gateshaw, near Kelso, the appearance of the highly illuminated minute globules of moisture was that of various strings of brilliantly coloured gems, disposed in radiating lines. Strings, as of the topaz, diamond, aquamarine, sapphire, emerald, and amethyst—were dispersed in splendid association over the surface,—whilst each series was, as if magically, altered into some other, by the slightest lateral movement of the eye of the observer.

In one case, where there was a small and irregular web cast over a portion of the gravel of the carriage drive before the house, the colours, which were rich in their clearness, were intermingled in splendid irregularity, as of a number of seed beads, formed of various precious gems, spread confusedly out upon an illuminated white ground.

In another case, and one, indeed, which was by no means of uncommon occurrence, the web of gossamer happened to be of a form and magnitude of surface, with an appropriate profusion of aqueous globules, which, when viewed in its proper position, and with a high degree of illumination, could not fail to remind one, when contemplating the singular beauty, of some of the most splendid clusters of stars as viewed by the gigantic telescope of the Earl of Rosse! In an instance observed at Gateshaw so recently as September 25th, when various little patches of dew-covered gossamer were scattered about the grass of the lawn, and illuminated by a brilliant morning sun,—one of these nebulous looking surfaces was examined of singular analogy, comparing small things with great, with the telescopic appearance of the magnificent nebula or star-cluster in the constellation of Hercules. Viewed when in the angle shewing a highly illuminated surface of colourless light, it presented an aggregation as of ten thousand resplendent diamond-like points, and, as it so happened, with here and there a larger dew-drop, on the proximate grass, appearing almost in the glory of a star of the first magnitude! As in the other cases, a slight alteration in the position of the eye, changed the previous white light of the dewy gossamer into a succession of the richest qualities of reddish orange, orange, gold yellow, straw yellow, diamond-like white, green, purple or violet. At the same time the blaze of white light of the adjoining grass-drop, participated in the analogous changes; sometimes, however, from its considerable size and nearness, presenting a series of tints from red to orange and yellow, or even white, within the same individual globule.

GATESHAW, September 26th, 1850.

Floods in India in 1849. By Dr BUIST of Bombay. Communicated by the Author.

The rainy season of 1849 was one of the most remarkable that has occurred in India within the present century. On the Western Ghauts no rain fell in May, and but little in June; and it was not till near the middle of July, or full six weeks after the usual time that the fall became general; in Goozerat, indeed, famine from extreme drought was apprehended till near the close of the month. On the 22d, 23d, and 24th of June, a violent atmospheric commotion occurred all over the country. On the second of these days the barometer fell almost unprecedentedly low at Calcutta, Madras, Lucknow, Hoshungabad, Trevandrum, Bombay, Kurrachee, and Aden, the first and last two places being 3000 miles apart; and we presume at all the intermediate stations, though from those named alone returns have been received. The depression of the mercury was infinitely greater than could have been looked for from the amount of storm which followed. At Aden and at Kurrachee rain seemed long promised, but none fell. A severe gale swept the upper part of the Bay of Bengal, extending to Arracan and Madras. The ships *Cabrass* and *Victoria* were lost in it, and many others endangered; the ship *Lord Dufferin* lost her helm, and was in great danger, on leaving Bombay harbour. On this occasion violent rain fell in the Jullundhur Doab, along the line of the Chenaub and Jhelum, at Simla, Delhi, Agra, and Meerut. At Broach eight inches fell in as many hours, and the fall seems to have extended all over India. From this date the barometer began suddenly and steadily to rise; on the 25th it had reached 29·722 at Calcutta, and on the 27th 29·716 at Bombay, having all at once sprung up nearly half an inch in two days at the former place, and above a third at the latter.

At this time plentiful showers occurred round Benares and Ghazeepore, when it faired up almost for nearly a couple of months, to the great detriment of the country.

At Calcutta, 3 inches of rain fell on the 27th, and 2·40 inches on the 1st, and again on the 9th July,—rain and fair weather prevailing day about. For eleven days on end not a drop seems to have fallen at Calcutta, and from the 9th to the 25th only two days of rain occurred, when 1·80 inches fell.

The rains at Calcutta had, notwithstanding, up to this time, fully reached their average, and there had been no month since the commencement of the year without showers. The quantity that had fallen during the three monsoon months was in all 34·28 inches; viz., for May 7·44; June 14·40; July 12·24. The total fall for the year had been 40·67; that of 1848, up to 1st August, 38·96. The total fall at Calcutta last year was 58·69.

During the first fortnight of the month we had at Bombay seven days wholly fair; on the other seven the rains were very light. On the 16th and 17th we had heavy falls, which now continued with little intermission. While a plentiful supply of rain was thus being provided for the Malabar coast as far north as Goozerat, all along the Ghauts, around Sholapore, and over a great part of Candeish, only a few showers had occurred over the Deccan. At Ahmedabad, so late as the 27th July, a famine was apprehended: kurbee, which sold last season at 60 bundles, was selling this at 16 to the rupee. The Saugor and Nurbudda territories were suffering still most severely. Around Deesa and along by Mount Aboo by Sehore, Ajmere, and Nusseerabad, and all over Rajpootana,—at Delhi, Meerut, Agra, and all along the north-west provinces,—such was the deficiency that a terrible scarcity and famine was apprehended. The barometer stood high, and the heat was excessive; and though there seemed frequent promises, there was no actual fall of rain worth notice anywhere. On the 22d, the first threatenings made their appearance. A hurricane swept the Jullundhur Doab, carrying everything before it. A similar gale levelled the barracks of Her Majesty's 32d with the ground: a kindred one destroyed the barracks at Ghazeepore. Heavy rain fell at Meerut, but did not reach Delhi, though it raged all around. A severe thunderstorm with rain occurred at Poona, and heavy showers fell at Ahmedabad: it poured in torrents at Bolarum. On the 25th, a tremendous burst occurred all over India. At Bombay, where it had been raining heavily before, the unprecedented fall of nearly a foot occurred, and 16 inches fell in three days. An Arab ship was dismasted half way across from Muscat. A heavy fall occurred at Poona and all over the Deccan, at Sholapore, Ahmednuggur, Surat, Ahmedabad, Agra, Meerut, and Delhi, reviving the hopes of the husbandman, and substituting the prospect of plenty for the apprehension of want. On the 25th and 26th it rained and blew violently at Phoonda Ghaut, the barometer falling to 27·924—the lowest it had been during the season. In the course of four days 26 inches of rain fell at the Ghaut; in the same time above forty fell at Mahabuleshwar. Violent rains occurred over the southern part of the Chinese empire in May and June; up to the middle of July the fall was heavy and the barometer low. On the 26th July one of the most furious storms of rain and thunder ever known occurred over the south of England.

The season along the north-west frontier, from this time forward, presented the most anomalous results. On the 3d of August the rain fell with the utmost violence all along the Malabar coast, and another period of unusual and general disturbance now made its appearance, just before the final drawing off of the rains; for at Bombay on the 4th, just as the monsoon had reached its full, the barometer suddenly rose by nearly a quarter of an inch in only six hours' time, the weather became showery and open, this state of matters extending at least 100 miles into the interior. On the 3d a severe storm occurred off the mouth of the Ganges, in which a large vessel belonging to the king of Burmah was lost. The first full moon, and the weather all over the country changed. On the 17th of August there seems to have a general fall of rain all over the country, though more moderate in amount than many of those which had previously occurred.

On the 27th July rain began to fall at Simla, and so continued almost without cessation up to the 7th August. On the 27th heavy rain fell at Wurzeerabad and Lahore, and a very large fall seems to have occurred. At Delhi, and so on to Benares, after the first downpour the rains became light and irregular. At Almorah during the first four days of August a very heavy fall occurred. At Allahabad scarcely a shower fell betwixt the 24th June and 4th August, when, on the 5th, a tremendous downpour occurred, and so continued for some days. Up to the middle of August scarcely a drop had fallen since the end of June and commencement of July, and the crops were completely burnt up; the river Burma was nearly dry, and at Jounpore the cultivators were endeavouring to keep their cattle alive with sugar-cane. While abundance of water was making its appearance on every side, at Ferozepore and all along the south-eastern bank of the Sutlej, a few casual showers were all that had occurred, the fear of famine beginning to become universal. Around Lahore and Mooltan, and so by the banks of the rivers, the country was completely inundated, while at Ferozepore the drought continued fierce and unmitigated. At Kurrachee, in Lower Scinde, where rain rarely appears, a heavy shower fell, and some thunder occurred on the 4th of August and again on the 16th August, the whole month of July having been thick and cloudy, with a few drops of fall every now and then.

Even with the limited information we possess, a multitude of singular facts are here disclosed to us, one of the most striking of which is the diversity in the state of the air in matter of humidity when the rains were at their wildest. Taking the crisis of the 22d June as an example, we find the wet and dry-bulb thermometers to have stood as follows at their maxima and minima at the following places:—

	Bombay.		Madras.		Aden.		Kurrachee.		Trevan-drum.		Hoshungabad.	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.
Dry.....	84	81	97	83	95	87	93	83	83	76	79	No return.
Wet....	81	78	78	75	76	78	84	80	80	75	77	No return.
Differ.	3	3	19	8	19	9	9	3	3	1	2	...

Here we have the air at Madras in the midst of frequent rain, though not the rainy season nearly as dry as it is with us during the fair weather; while at Aden, June seems the driest. January one of the wettest months of the year. This is one of the most important conditions of climate: observations with the wet-bulb are almost as easily made as with the dry-bulb thermometer—they ought on no account ever to be omitted.

The following rain returns will shew the amount of fall, in inches, for May, June, and July, on this side of India :—

	Bombay.	Poona.	Surat.	Nassick.	Asseer-ghur.	Ahmed-abad.	Phoonda Ghaut.	Maha-bleshwar.	Paunch-ghunny.	Calcutta.
May....	...	0·405	...	. .	0·23	2·03	...	...	...	7·44
June...	22·80	9·055	11·16	8·63	5·45	4·16	50·00	59·90	...	14·43
July....	51·60	6·425	19·00	7·03	16·31	7·62	83·00	89·24	...	12·24
Total...	74·40	15·885	30·16	15·66	21·99	13·75	133·00	149·14	11·95*	34·11

At Calcutta, the fall in April was 1·25; May, 6·00; June, 13·00; July, up to 24th, our latest returns, 8·25: total 28·50.

Hailstorms usually occur in our dry—most frequently in our hot—weather in India: the most severe hailstorm yet recorded for the year 1849, was that at Jaulnah on the 15th January, though many of much severity happened all over lower Bengal in the months of April and May. Those of the 3d of the month last named prevailed all over India from Ootacamund to Peshawur. A very severe hailstorm occurred at Bassein on the 2d June. The Malwa hailstorm of the 6th and 7th June was unusually late for the season. We now find heavy hail falling at Mahabuleshwar for three days on end on the 27th, 28th, and 29th July, during the very wettest of the season, without thunder or lightning, or storm.

* Up to 15th July.

These results have been thrown together with a view of conveying all the information that can be collected from all parts of India over the heaviest of the rainy season; and, imperfect as they are, compared to what they might readily be made, we venture to say that a much larger amount of information has been conveyed by them than is to be found in any single paper or in any similar space. Papers on similar subjects are now issued by the Greenwich Observatory, quarterly, like the Chancellor's accounts; and the government of India would be conferring a service on the public were the example set at home to be copied by them. They may rely on it that the rumours of those interested in spreading alarm are much more gloomy in the most unfortunate seasons than anything they can set forth: and that on such occasions as these, truth is not only much more satisfactory and salutary, but far more cheering than fiction. The space required for the authorities on which our statements are based, is more than we can afford: the great bulk of them are from the newspapers—we have no doubt whatever of their perfect authenticity: our only cause of regret is that they should be so few, and that so large an amount of information should be constantly in the way of being collected by the various departments under Government—especially by the medical—from which so very little benefit is derived. All the trouble is taken, but no gain whatever arises from it to any one.

The month of August was generally open all over the country: from the 17th, indeed, along the western seaboard the monsoon appeared to have been over, when on the 1st of September it recommenced with double fury, no less than ten inches having fallen at Bombay in the course of the week—betwixt twenty and thirty inches fell on the seaboard, and consequently about double this on the mountains in the course of the month, the fall along the lowlands having been betwixt 130 and 150 for the monsoon, or nearly double the average. On the Eastern coast, again, from lat. 15° S. showers fell during the season usually fair with them, the dry weather on the Coromandel Coast corresponding with the rains in June, July, August, and September in the other parts of India—their own rainy season, November, December, and January, was one of the most defective ever known throughout the Madras presidency.

About the beginning, and again near the middle of August, a tremendous fall appears to have occurred along the range of mountains bordering the northern and north-western frontier of the Punjaub. The Indus, Jheelum, Chenab, and Ravee came down in unrestrainable fury, and burst through all their boundaries, deluging the country as they went. On the 3d August the cantonments of Wuzeerabad on the Chenab were entirely flooded, and the troops required to be moved. This, however, was a trifling matter in comparison to what followed a fortnight afterwards. A tremendous fall occurred in the mountains in Cashmere, from which the Jheelum derives its waters. The inundation which followed deluged the plains below the Salt Range. At Pind Dadun Khan the government salt-stores were washed away—at Shahpore, a little further down, the cantonments were more swept away, and the troops compelled to remove to a distance of five miles. The flood gathered force as it advanced by a heavy fall of rain, about four inches having been measured in the night betwixt the 15th and 16th, at the usually dry station of Mooltan. About eighty miles above this the river burst all its embankments, and laid the whole country under water. The batteries, outworks, and other works of Mooltan, which a year before had for four months defeated all the efforts of our artillery, melted into the flood. On the 16th the magnificent domes fell, and at seven o'clock on the morning of the 17th the enormous cupola of the Bahawul Huk came thundering to the ground with a noise like the explosion of a stupendous mine. The whole structures were built of unburnt brick,—no such flood had ever been known to occur. The effects of the deluge were felt at Sukkur, and all down the course of the Indus.

The first burst of rain during the first two weeks of September occasioned a second series of floods further to the south. The town of Cambay was completely inundated by the flooding of the Myhee on the 19th, in conjunction with a tide of almost unprecedented height: sixty houses fell—hundreds of others sustained most serious damage. To the south of Surat no river of any size finds its way to the Western Ocean, though the vast streams which discharge themselves in the Bay of Bengal have their sources in the Ghauts close by, and are of course affected by the western rains. On the 10th the Godanery rose on the Nizam's dominions to an unusual height—the river Mossa, which takes its rise to the westward of Hyderabad, swollen by the rains which had prevailed for a fortnight all over the country burst through all its banks. On the 12th it burst into the city, washing down the walls, levelling the houses, and deluging the neighbouring cantonments. A rise of a few feet more would have choked up the bunds and most likely have carried them away. The torrent was awful—it was an immense, resistless mass of turbulent waters, threatening to engulph everything within its reach. It was a beautiful sight to see so slender a fabric as the bridge built by Major Oliphant spanning the flood, the waves looked like huge giants rushing forward to lash its sides. The water reached to within six feet of the quay, but did no harm. The freshes visited Coringa at the debouchure of the river, and nearly inundated the town. The house of the collector, the highest in the place, was three feet under water, and all the rest were submerged: the loss of property was immense.

On the Fragments of a Bronze Furnace (supposed to be Phœnician) discovered near St Michael's Mount,—the Itkin *of Diodorus Siculus.* By RICHARD EDMONDS jun., Esq.*

I use the word *Iktin* advisedly, having been informed by a friend, who has consulted the best editions of the original work at the British Museum, that this is the word used by Diodorus, and not *Iktis;* for which last name, although universally adopted, the only authorities are his translators, French and English as well as Latin, who as *Ictin* happens to be the accusative case,† have assumed that the nominative was *Iktis*, whereas (admitting the word to be declinable) *Iktin* might be the nominative with as much propriety as *Ictis*. Moreover, the most ancient name by which the Mount was known, after it became a religious cell, was *Dinsell*, or *Dynsull*,‡ a mere corruption probably of *Iktin-cell*, or *'Ktin-cell*.

As *Ictin* thus appears to have been the ancient name of the Mount, let us dwell for a moment on its etymology. *Ik* being the Cornish word for *cove* or *port*, *Ik-tin* signifies *port-tin* or (according to the English idiom) *tin port*§—a name as appropriate and at the same time as indefinite as could have been given by the Phœnicians, who sought to conceal the place from whence they procured their tin. It is generally supposed that they obtained their tin first from Spain: when they had discovered it in Mount's Bay, and had exported it from the *Mount*, they might, in order to distinguish it from their Spanish tin, have called it *Bre tin* (Mount tin), *bre* being the Cornish word for *mount*. Now *Bretin*, by our continental neighbours, is pronounced *Bretagne*—the very name of that part of France which is called after Britain; so that *Britain* and *Bretagne* are apparently mere different pronunciations of *Bretin*.|| The name by which tin was known amongst the Phœnicians and Chaldeans has undergone considerable changes since its intro-

---

* *Vid.* Penzance Natural History, and Antiquarian Society's report for 1849.

† "εις την νῆτον προκειμενην μεν της Βρεττανικης ονομαζομενην δε 'Ικτιν."—*to an island close to Britain named Iktin.* Lib. 5. c. 22.

‡ Carew's Survey of Cornwall, edited by Lord de Dunstanville, p. 376.

§ In the celtic, it mattered not whether *ik* or *tin* were placed first. The word *porth*, for example, which is synonymous with *ik*, sometimes *follows* the name to which it is joined, as in *Perran-porth*, although generally it *precedes* it, as in *Porth-curnow*.

|| The new derivation of *Britain* will not appear improbable, when we consider that the most striking object, as well as chief place, in the very small part of Britain known to the Tyrians, must have been the Mount, then called *Bré*. The learned Bochart derived the name *Bretanikee*, the Greek word for *Britain*, from the Phœnician or Hebrew words *baratanac* (the land of tin.)

duction into European languages. While the Saxon, Dutch, and Danish word is the same as our own, the Swedish word is *tenn*, the German *zinn*, the French *etain*, the Latin *stannum*, the Irish *stan*, the Cornish *stean*, the Armonic *stean* and also *staen*, the letter *s* in each of the last four words being probably a mere prefix, as in the modern word *sneeze* for *neese*.*

It may be inferred, from the account given by Diodorus, that no smelting-works existed in the ancient *Iktin:* and we may safely conclude that St Michael's Mount never had any; for they are invariably erected near streams, and no stream is found there. The stream nearest it is that flowing into the sea at Marazion; and its banks must have been one of the most convenient spots in Mount's Bay for a smelting establishment. On this very spot, traces of such an establishment have been discovered. The stream being lately diverted, flowed westward to a considerable distance along the base of the adjoining sand-hillock, rapidly undermining it and washing away large portions. In sections thus made, I saw, at the depth of between 12 and 20 feet beneath the surface, the remains of ancient walls rudely built of unhewn stones mixed with clay, and near them great quantities of ashes, charcoal, and slag, or the vitrified refuse of smelted ores, such as may be seen near any tin-smelting house at the present day—grains of tin being frequently imbedded in the slag. Some very ancient broken pottery, of rude manufacture, was also found, and much brick. But the most extraordinary discovery which my nephew and myself made when we had removed a portion of the sand, within a few inches of one of the walls, was two fragments of a bronze vessel resting on a layer of charcoal. A considerable portion of the charcoal had combined with the metal during the lapse of ages, and a beautiful green substance had resulted, closely resembling, and no doubt identical with, malachite, or the carbonate of copper. The fragments were each about six inches long, four wide, and only about 1-20th of an inch thick, having been apparently parts of the circular top of a vessel three feet in diameter, the mouth being bent back into a horizontal rim three-quarters of an inch broad. The charcoal adhering to the vessel was exclusively on the outside. It seems highly probable that this furnace was brought hither by the Phœnicians; for it is recorded by Strabo that they furnished us with "earthenware, salt, and brass (or bronze) utensils" (χαλκωματα †) in exchange for our tin, lead, and hides. Cæsar, too, has stated, that the "brass" (or bronze) which the Britons used was imported. (*Ære utuntur importato.*‡) Robert Hunt, Esq., at whose request I have presented a fragment to the

* See Job xli. 18.
† Geograph. iii. 8. See Ezra viii. 27, and Ezek. xxvii. 13.
‡ De Bello Gallico, V. 10.

Museum of Economic Geology, has been so kind as to analyze a small portion of the metal, the following being the result :—

Weight, before analysis, 25 grains.

	Grs.
Copper, ...	18·0
Tin, ...	2·25
Iron, ...	1·0
Loss, as carbonic acid and oxygen—the copper being partially in the state of carbonate, and much of the tin in oxide, . . .	3·0
Earthy matter, ...	0·75
	25·0

The supposition that these remains have been buried for at least 2000 years, and that the sand has occupied all that period in covering them to their present depth, agrees with my suggestion to the Royal Geological Society of Cornwall, in 1846, respecting the origin of our sand-hillocks; which appear to have proceeded from the sands drifted from the sea-shore by violent winds, and to have been " for the most part accumulated imperceptibly upon a continuously growing vegetable surface, the deposits during a single storm being too slight to cover the herbage or to check its growth, except occasionally when they were sufficiently copious to bury it almost entirely." These very ancient ruins, therefore, with the fragments of a bronze vessel, the broken ancient pottery, the abundance of ashes, charcoal, and slag, all covered by the sands of many centuries, appear to indicate the very spot where, as Diodorus relates, the tin was purified previous to its conveyance in carts to the neighbouring island during the recesses of the tide.

Another argument has suggested itself in favour of this conclusion; the merchants who first purchased the ores and purified them for sale are generally supposed to have been Jews; indeed, the rude smelting-pits, containing the dross of tin and charcoal, which are occasionally found in Cornwall, and were no doubt commonly used prior to the erection of any general smelting establishment, are everywhere called Jews' houses.* There is, too, a tradition, that our stream-works were " first wrought by the Jews with pickaxes of holm, box, and hartshorn," tools frequently " found amongst the rubble of such works;† but as soon as the natives had acquired the

* The practice, " was to dig a hole in the ground and throw the tin ore on a charcoal fire, which probably was excited by a bellows." This was attended with " an undue consumption of fuel and a great loss in the produce of the ores, as the more stubborn parts would not give way to that degree of heat which by this method they were able to apply." To this cause principally Pryce ascribes the destruction of our woods. (*Pryce's Mineralogia Cornubiensis*, p. 281.)

† Carew's Survey of Cornwall, p. 26. Norden, p. 12. " Scawen in his MS.

art of mining, the Jews probably purchased the ores from them, and established a depôt and market near the recently discovered smelting-works. Now, it is remarkable that the town on whose borders these works were erected, is considered the most ancient in the county ;* and what is still more remarkable, each of its two names is a record of the fact of its having been a market of the Jews. The names are *Market-jew* and *Marazion*. In the latter, *Mara* is a contraction of *Margha* "market," and *Sion* is the Hebrew for "mount." Assuming, therefore, that it was originally inhabited by the Jewish tin-merchants, it was natural for *them* to call it *Marghasion* (the name on the Town Seal) or the "Mount-market," from its proximity to, and dependence on, the Mount ; whilst the native miners, thinking more of the Jews, to whom they were carrying their ores, would call it *Market-jew*.

PENZANCE, 6th October 1849.

Abstract of a Memoir on the Metalliferous (Gold) Deposits of Brazil. By WILLIAM JORY HENWOOD, F.R.S., F.G.S., &c., &c., &c.† Communicated by the Author.

The gold-bearing strata consist of granite talcose and clay slates, and a granular rock of quartz and talc, locally called Itacolumite,‡ in which the latter is sometimes replaced by oxide of iron. These are followed by the *Jacotinga*,§ the principal auriferous rock, which is for the most part composed of specular iron-ore and oixde of manganese, but sometimes contains talc, mica, and quartz also. A rock very closely resembling that beneath the *Jacotinga*, but generally rather less quartzose, succeeds : and this is overlaid in many places by calcareous strata. No organic remains have yet been found in any of these formations. The gold is either disseminated through the rock, and in the short unconnected strings and masses in and forming integral parts of the strata—in much the same manner as tin ore occurs at Carclaze, and in the small veins at Balleswidden, Beam, St Agnes, and Drake Walls ; or disposed in veins or vein-like masses as it is at Candonga, Morro Velho, Gongo Soco, Cocaës,

---

adds that the Jews, as well as Phœnicians, were very ancient traders in Phœnician ships." Buller's Account of St Just, p. 5.

* Davies Gilbert's Cornwall, vol. ii., p. 215.

† Read before the Royal Geological Society of Cornwall, 27th September 1850.

‡ From the mountain Itacolumy near Ouro Preto, which is composed of it.

§ From its resemblance in colour to the plumage of a well-known Brazilian game-bird, so-called.

and Bananal,—or again, in a rounded, sandy, or gravelly state, mixed with other detrital matter, in which case, as in that of our stream-tin, the quality is far superior to that of the metal obtained from mines. A fourth mode of occurrence owes its origin to the workings on the other three, for it consists of the finer and lighter particles which escape during the extraction and cleaning of the gold obtained from the strata and veins, and which are often carried by the rivers several miles before they subside. This is obtained from the present beds of rivers; and after heavy floods, it is also collected from the grass and brush-wood which clothe their banks; but it is wrought only by the very poorest classes, and seldom yields them more than a very few pence a day. A rich sample of gold taken from the crop of a duck which fed in one of those streams was exhibited, and this, though very rare, is not a sole instance of the kind. The writer once saw the sand and earth scraped by children from between the paving stones in the street of Itabira for sake of the gold they contained. The mine of Gongo Soco, worked in the *Jacotinga* formation by an English association, afforded its riches so near the surface, that the extraction of gold was begun on the third day of its prosecution, and it continues to be wrought, though on a very reduced scale, to the present time. In the month of September 1829, there were 759 lbs. of gold obtained, of which 296 lbs. (or nearly £12,000 worth) were extracted in two days; and during twenty-four years, more than 33,000 lbs. weight, worth about one million and a quarter sterling, have been taken out, and yielded a very considerable profit.—The mine of Morro Velho, also carried on by an English company, at present yields auriferous pyrites only; but though it contains only about half an ounce of gold in the ton of ore, it is nevertheless so extensively wrought that it gives from 200 to 250 lbs. of gold a month, and has for several years past left a large profit to the adventurers.

The proportion of gold extracted from the *strata* he estimates at two-fifths of the whole.

The proportion of gold extracted from the *veins* he estimates at one-half the whole.

The proportion of gold extracted from *stream-works* and beds of rivers he estimates at one-tenth of the whole.

The first discovery of gold known to the Portuguese authorities was in 1695, and from that time to the end of last year, the writer calculates by the aid of Eschwege's work on Brazil, and by assistance of the Government officers, that sixty-three millions sterling worth of gold had been extracted from the Brazilian gold workings. To the end of 1846 (the latest returns he had access to), the Russian gold washing had yielded about twenty millions; and Sir Roderick Impey Murchison considers the returns from California as one million and a half per annum. The latest Russian accounts shew a produce of more than three millions annually, and they, as well as the Californian, are still on the increase. The value of Brazilian workings seems never to have much exceeded one million a year, and it has for a long time been on the decline; the present produce is calculated by the best authorities at about 6,000 or 7,000 lbs. of gold per annum, worth from £220,000 to £270,000; of which about one-half is extracted from mines worked by British skill and capital.

The gold of Candonga, Gongo, and Bananal is alloyed with Palladium, as well as with some silver, and a little platina; at Fazendao it is mixed with native copper, and this is probably the case in several other mines; at Morro Sao Vicente, large quantities of Tellurium are mixed with the gold; and the sulphuret of bismuth was occasionally found at Catta Branca. Crystallized gold is rare, but the little which occurs is chiefly obtained from the present beds of rivers; whence, like our own crystalline minerals, it is doubtless derived from the shallower portions of the veins or strata. Iron ore of the richest description occurs in inexhaustible abundance; and the only circumstance which can interfere with that metal becoming hereafter the staple of Brazil, is the indiscriminate destruction of the forests, and the absence of coal.

The author never saw a regular cross-vein in any part of Brazil, but was informed by the intelligent German engineer, Mr Von Helmreichen, that wide granitic cross-veins traversed the gold vein at Candonga.—With the assistance of Eschwege's Statistical Accounts, he estimates the number of labourers employed in extracting gold at about 13,000, of whom perhaps 10,000 are slaves, and the remainder freemen; and, comparing their numbers with the produce of their labour before-mentioned, it appears that each person collects on an average only about twenty pounds sterling worth of gold in the year. So small a return must long since have led to the abandonment of this pursuit were it not for the extremely cheap manner in which the natives and their slaves are supported;—and for the stimulus afforded by the immense prizes even yet found by the more fortunate miners. Still, with every possible allowance, it appears that capital may be invested in our own mines with far greater chances of success than are offered by the Brazilian gold workings.

About 2,000 slaves are employed in the Anglo Brazilian mines; of whom perhaps 1,200 are the property of the companies; the remainder are hired from native slave-owners; they are all well fed, clothed, and housed. But notwithstanding our laws prohibit British subjects from *purchasing* negroes, it is deeply to be lamented that they are silent on the subject of *hiring;* a circumstance still taken ample advantage of by too many of our countrymen, who thus supply themselves with slave labour, and thereby give the African slaver countenance and encouragement; whilst they as directly contribute to the profit of his abominable traffic as if they had been actually buyers.

A short experience will satisfy an unprejudiced observer that the emancipation of the slaves without previous training in self-control, and in the arts and duties of civilized life, is rather inflicting mischief by setting at large a savage who will return to barbarism, than conferring a benefit or raising a fellow creature in the scale of humanity. The author, soon after his arrival, established a place of secure deposit for those blacks who wished to economize their earnings; founded a system of rewards amongst them for the finest poultry and pigs;—for the most neatly kept gardens;—the cleanest houses, and for the best general conduct;—opened a school for the negro children, and added to the number he found already learning handicrafts.—A strong spirit of emulation was soon excited amongst them; and subsequent observation shewed that many of the slaves might with equal safety and advantage be entrusted with absolute freedom. Several adults were therefore emancipated; and the excellence of their subsequent conduct gave gratifying proof that the care and culture bestowed on them had not been in vain. A similar boon was also conferred on many children of parents who, though themselves still slaves,—gave evidence that their offspring would be brought up in habits of order, sobriety, and industry.—Where the dominant race counts less than one-fourth of the number of its captives, a social revolution cannot be far distant; and we hope the free population of so vast an empire will see and profit by an example, which, if regarded in time, may at the eventful period peaceably effect that change, which must be otherwise brought about by a catastrophe too horrible to contemplate.—It is a fact well known in the interior of Brazil that the greater scarcity and higher price of slaves now than formerly, ensures that unfortunate race much better treatment at present than they received when their loss could be supplied from the market, at the low rate which anciently prevailed; a fact acknowledged by every native slave-owner. This scarcity, greater value and increased comfort, are all the results of our blockade; and thus the blessing of British humanity is daily felt by the captive in the remotest corner of Brazil.

This paper was of very great length, and contained numerous descriptions of mines and scenery; it was illustrated by an extensive and beautiful suite of gold specimens on the table.

On the Dynamic Equivalent of Current Electricity, and on a fixed Scale for Electromotive Force in Galvanometry. By Mr WILLIAM PETRIE. Communicated by the Author.

The dynamic value of a current of voltaic electricity is represented by the product of the rate at which electro-chemical action is taking place at any cross section of the current (in other words, the *quantity of the current*), and the electro-motive force with which the current is sustained, which may be briefly termed its *energy* or *intensity* (provided the idea of *quantity* be kept distinct from this.) The first object is to secure such units of comparison for both these elements as should be at all times recoverable. This is given, in respect of quantity, by the rate of chemical action, and by the atomic weights. In respect of the intensity of the current, we have no such fixed data, and the intensity of most voltaic arrangements cannot be relied on as constants for comparison.

But the elements of Daniell's battery, and those of nitric acid batteries with negative surface of platinum, carbon, or cast-iron, give an electro-motive force or intensity that can be recovered with considerable exactitude, if uniformity of circumstances, materials, &c., be moderately attended to; these, therefore, may be used to give a fixed and recoverable point in a galvanometric scale of intensity.

Now, it so happens, that if we assume the degrees of the scale to be of such a size that the intensity of Daniell's (standard) elements shall be 60°, the temperature being 70° Fahrenheit, that of nitric acid batteries will be from 100 to 112 of the same degrees. I have, therefore, always used this scale, to which all other voltaic arrangements can be referred (as shewn in the table at the end of this paper), and which, I would suggest, would be most conveniently used in assigning the electro-motive power of electric currents from any source.

The mean result of careful experiments, tried directly and conversely, is, that a voltaic current of one unit in quantity (or that from one grain of zinc electro-oxydized per minute), and of 100° intensity, represents a dynamic force of 302½ *pounds raised one foot high per minute.* This datum is of great interest, as a scientific truth, in connection with the other correlative agents of nature,—heat, electricity, light, chemical affinities, neuralgic power, &c., most of which we may hope soon to see reduced to a mutually comparable relation, in terms of the great centre and medium of comparison, *Mechanical Force.*

Table of the Relative and Absolute Powers of various Galvanic Arrangements, &c., shewing the Electric Current circulated by them, after the surfaces of the elements have been in action for several hours *with continuous supplies of liquids, the Temperature being* 70°.

Description of the Galvanic arrangement.	Square inches of acting negative surface, requisite to circulate one unit of Intensity.	Permanent Intensity while working.
Hard cast-iron, with nitric acid, and zinc with dil. sulph. acid, *warm*, say 80°, ...	$4\frac{1}{3}$	100°
Grove's.—Platinum, with nitric acid, and ditto, ...	$4\frac{1}{3}$	102°
The Author's.—Carbon, with nitric acid, and unamalgamated zinc, with a saline solution, warm, 80°, .	$4\frac{3}{4}$	112°
Daniell's.—Copper, with sulph. copper, and amalg. zinc, with dil. sulph. acid,	16	60°
Smee's.—Platinized silver and dilut. sulph. acid, with amalg. zinc, .	$5\frac{1}{4}$	Average 36° but very variable.
Plain copper, with dil. sulph. acid and amalg. zinc, . .	52	18°
Plain lead, with ditto, . .	104	$23\frac{1}{2}$°
Thermo-electric pairs, bars of bismuth and antimony, one inch long, diff. of temperature of their ends, 90°, . .	Section of each metal $\frac{2}{9}$ sq. in.	$\frac{1}{4}$°

These data have been found very useful in determining the best proportions of batteries for practical purposes.

On the Application of Electricity and Heat, as Moving Powers. By Mr William Petrie. Communicated by the Author.

From the dynamic equivalent of electricity already given (p. 64), we can infer an important fact, that *one horse power* is the *theoretic* or *absolute dynamic force possessed by a cur-*

rent of electricity derived from the consumption of 1·56 lb. *of zinc per hour*, in a Daniell's battery. But the best electro-magnetic engine that we can hope to see constructed, cannot be expected to give more than half or one-fourth of this power; in any case, we see here the limit of power which no perfection of apparatus can make it exceed. The peculiar mode in which the electric current produces dynamic effects, has led to much miscalculation respecting the power obtainable from it. In every sort of electric engine, the material to which the neighbouring current gives motion, whether it be another moveable current, or, what is more usual, a magnetic body, is impelled in one direction with a constant force; and this force, whether it be attraction, repulsion, or deflection, is, like the power of gravity, *sensibly constant* at all velocities, however fast the body recedes before the action of the force, provided only the same quantity (per minute) of electric current be maintained. This is quite different from the action of steam-power, in which the faster the piston moves the greater is the volume of steam per minute that must be supplied to move it, or else the less will be the power with which it moves.

This fact, then, that the force with which an electric current of a given "quantity" moves the machine, is the same at any velocity of motion, bears no analogy to the case of steam, but would indicate that the dynamic result obtainable from a given electric current might be indefinitely great; and so it would be, were it not that the part moved always tends to *induce a current* in the wire in the *reversed direction;* and this inducing influence, which increases with the velocity of motion, conflicts with the original current, and reduces *its quantity*, and consequently reduces *the power of the motion*, as well as the consumption of materials in the battery.

Some have imagined that possible alterations in the parts of the machine, or in its mode of action, would avoid the evil, or might even make the induced current to flow *with* the primary current, instead of *against* it. The impossibility of this, though not readily proved in detail, can be at once proved by reference to general principles; it would, if true, be a *creation* of dynamic force, the evolving of an *unlimited force* from a *limited source.* The tendency to an opposing induced current in the primary wire must therefore be involved in the *very principle* of the system, so that no ingenuity can ever get rid of the retarding influence of the induced action; the only way to overcome its power, so as to maintain the primary current from falling below a given rate or quantity, when the machine is allowed to attain rapid motion, is to increase the *electro-motive power of the battery*, the *intensity* (not the *quantity*) of the current, so that it shall be less affected by the opposing induction.

The practical importance of these not altogether unknown truths may justify this particular notice of them. For want of a clearer apprehension of them, inventors of considerable attainments have misapprehended the direction in which improvements were to be made, and much ingenuity and means have been wasted.

Some of the best electro-magnetic machines of other inventors, which have been properly tested by the author and others, on a practically useful scale, have only given a power at the rate of 50 or 60 lb. of zinc, per horse power, per hour. The smallness of this power, in comparison with the absolute value of the current (1·56 lb. of zinc, per horse power, per hour) should not cause surprise, if we consider the present case of steam, after many years of improvement.

According to the determinations of Joule and Rankine on heat, 1 lb. of water raised 1° of temperature, is equivalent to 700 lb. weight raised 1 foot; and assuming, from the best experiments on the subject, that the combustion of 1 lb. of carbon will afford as much heat as 1° of temperature in 15,000 lb. of water, we find that one horse power is the theoretic or absolute dynamic force possessed by the heat due to the combustion of $\frac{1}{6}$ of a pound of carbon per hour. This is only $\frac{1}{15}$ of the consumption of the most perfect steam-engines yet constructed, and less than $\frac{1}{100}$ part of the consumption of many railway locomotives. This, by the way, is an instructive fact, shewing *how far* we yet are from perfection in our means of obtaining moving power from heat, and what great rewards may yet await the exercise of inventive genius in this department. Moreover, in the face of such facts, we need not wonder that we have, as yet, only obtained $\frac{1}{32}$ part of the power possessed by electricity.

But it is to be remembered that there is a far better chance of obtaining a larger portion of the theoretic power from electricity than from heat, owing to the characters of the agents. Let us attempt, in a few words, the explanation of the reasons. The power of heat is developed, not by *retaining* it as heat in a gaseous or liquid element, but by converting some of it into power, during the *process of diluting* or *diffusing* the heat (from a state of intensity or concentration in a comparatively *small* quantity of matter, such as the air of the furnace and surface of the coke), by imparting it to other colder matter. Now this process cannot be carried on with the effect of obtaining power through the greater part of the range of temperature, commencing at the heat of the air at the moment of contact with the incandescent coal, but can only commence at the comparatively low temperature at which it is necessary to keep the apparatus (whether steam-engine or air-engine) in which the heat is converted into power. For example, it is obvious that the heat of a cubic foot of air at 20,000° temperature (and the primary temperature must be far higher), will generate scarcely any more steam in a boiler than 10 cubic feet of air at 2000°; and yet a large extra amount of power might be obtained (had we materials to stand the heat) out of the process of transferring the 20,000° of heat in 1 cubic foot into the 10 cubic feet at 2000°; and the large portion of this heat, which was not yet rendered latent by the process, could be again used, as at first supposed, with almost as much proportional efficiency as if it were still in the most concentrated state. This consideration accounts for the wide discrepancy between the theoretic power due to heat and the power which is practically obtained from even the best Cornish engines; in which, nevertheless, *scarce any* heat is allowed to escape to waste, except in the very diluted state of the warm condensation-water. The above considerations may also indicate the direction in which inventive genius must work, in order to obtain any greatly increased results.

In the case of electricity, however, there is no analogous difficulty; but we have instead the difficulty and expense of developing current electricity by the chemical actions now requisite. If carbon could be burnt or oxidized by the air, directly or indirectly, so as to produce electricity instead of heat, 1 lb. of it would go as far as 9·36 lb. of zinc in a Daniell's battery, chiefly because there are as many atoms in 1 lb. of carbon as in 5¼ lb. of zinc; and partly because the affinity (for oxygen) of each atom of incandescent carbon is greater than that of an atom of cold zinc, minus the affinity of the hydrogen for the oxygen in the water of the battery. Apart, however, from such prospects of improved means of obtaining electricity, its favourable feature, on the other hand, in comparison with heat, is the reasonable expectation that some form of apparatus may obtain from electricity a considerable portion of the power which I have above determined to be the dynamic equivalent of the electric current.

On the Intensity of Sound in the Rarified Air of High Mountains. By CH. MARTINS, D.M. & S. Communicated by the Author.

In nature, where the artificial conditions of the laboratory are wanting, the most simple experiments in physics, and apparently the most conclusive, are found complicated with new elements and unforeseen difficulties which change or modify the conclusions which we are tempted to deduce from them. The following results are a striking proof of the truth of this statement; and I may express the hope that they will draw the attention of physicists to the still unknown causes which produce a variation of the intensity of sound in the open air.

So early as 1706, Hauksbee demonstrated, by experiments made in the fields, that the sound of a bell, under a receiver, is so much the stronger as the air is denser, and that it becomes weaker in proportion as the air is rarified.* Until

* An account of an experiment made at a meeting of the Royal Society of Gresham College, upon the propagation of sound in condensed air, together

this time, however, no rigorous attempt had been made to estimate this variation, which already complicated the first experiments made on the velocity of sound. When Lacaille, Maraldi, and Cassini de Thury, attempted to measure the velocity of sound, the last, placed at the station of Dammartin, heard very well for several days an eight-pounder fired from the top of Montmartre, the distance being 31,337 metres.* At Cayenne, La Condamine heard the report of a twelve-pounder placed 39,430 metres off.† On the other hand, Godin and Georges Juan, co-labourers with La Condamine, did not hear a nine-pounder at a distance of 37,031 metres ;‡ the two stations being above the plain of Quito, the one Gonapoli, 4110 metres, the other Pamba-Marca, 3009 metres above the level of the sea.§ The experiment was repeated upon the plain of Quito at an altitude of 2900 metres; the same cannon, placed at a distance of 20,500 metres, was heard, though very faintly.‖ So that, upon the plain of Quito, the explosion of a nine-pounder appeared weaker at a distance of 20,500 metres than that of an eight-pounder 31,300 metres off, in the environs of Paris.

The intensity of the sound depends on the density of the air at the place of primitive disturbance, and not on that of the strata traversed by it, nor on that of the air surrounding the hearer.¶ The following trials furnish a proof of this. When M. Bravais and I made our experiments upon the velocity of sound ascending and descending, between Brienz and the summit of the Faulhorn, we employed two mortars of exactly the same fount.** In the first experiments we gave them the same charge, but the sound created in the air, at an altitude of 2682 metres, was much weaker than that

---

with a repetition of the same in the open field.—*Philosophical Transactions*, vol. xxiv. p. 1902.

* *Mémoires de l'Académie des Sciences*, année 1738, p. 128.

† *Relation abrégée d'un voyage fait dans l'intérieur de l'Amerique.* (*Mémoires de l'Académie des Sciences*, année 1745, p. 488.)

‡ *Journal du Voyage, fait par ordre du Roi à l'équateur*, t. i. p. 36.

§ *De la figure de la Terre*, par Bouguer, p. 124.

‖ *Journal du Voyage*, &c., t. i. p. 98.

¶ Poisson. *Traité de Mécanique*, t. ii. p. 706.

** *Annales de Chimie et de Physique*, 3e série, t. xiii. p. 1 ; 1845.

produced 2117 metres lower. To equalize the two sounds, it was necessary to charge the lower mortar with 75 grammes of powder, and the upper one with 90 grammes.

The accounts of the ascents of the highest mountains contain some observations upon the weakening of the sound, but they often contradict each other. On the top of Mont Blanc, at 4810 metres above the sea, the sounds, according to Saussure, were remarkably weak. A pistol shot made no more noise than a china-cracker does in a room.* Mr Auldjo, being upon the same summit, cut the string retaining the cork of a bottle of champagne; the cork was projected to a distance, but the noise was scarcely sensible: he adds, that the sounds of voices seemed to him weakened. Mr Fellows, who made the ascent of Mont Blanc in the same year, is still more explicit: he says, that having desired the guides who accompanied him to sing the *ranz des Vaches*, they could never succeed, because they could not hear each other.† This assertion is evidently exaggerated. Placed on the summit of Mont Blanc, M. Bravais, M. Lepileur, and I, heard very distinctly the guides speaking to each other near the rock la Tourette, distant about 400 metres from the top, and reciprocally our guides also heard our voices when we conversed together. At fifteen or twenty paces, M. Lepileur remarked the noise I made when tapping with a lead pencil upon the metallic slider of my barometer.‡

Desiring to acquire some ideas more positive upon this subject, I sought the means of obtaining a continuous sound of a constant intensity, and producible at will: this I found in one of M. Marloye's diapasons, mounted upon a hollow box of very dry deal, parallelopipedal in form and 0^{m} 305^{mm} long, by 0^{m} 065^{mm} broad. The box was closed on one side and open on the other; the diapason sounded ut_3, which has 512 vibrations in a second. In a state of repose the separa-

---

* *Voyages dans les Alps*, p. 220.

† Rey, Influence sur le corps humain des ascensions sur les hautes montagnes (*Revue Médicale*, 1842, t. iv. p. 343.)

‡ Lepileur, Mémoire sur les phénomènes physiologiques qu'on observe en s'elevant sur les hautes montagnes (*Revue Médicale* 1845.)

tion of the branches of the diapason was 5^{mm} 6 and 8 millemetres when the wooden cylinder employed to put them in motion was placed between them. With a sound having always the same intensity in air of equal density, it is evident that the variable distance at which it ceases to be perceptible in mediums of different densities, should give us a measure of the variations of this intensity.

The agitation of the air complicates these experiments. Its influence has been successively studied by M. de Holdat at Nancy,* and by M. de la Roche at Paris.† They found that the limit of distance for hearing is increased or diminished for a person having the wind from or to the origin of the sound. But both agree in affirming that the sound is heard at the greatest possible distance in air at rest; the noise from the wind, come from what part it may, interfering with the perception of the sound. Our experiments having always been made during calm weather, or during a slight intermittent breeze which permitted us to choose the intervals of repose, we shall not occupy ourselves with this complication. We had, besides, two diapasons that we sounded alternately: if, then, the wind had favoured the hearing for one, it would have hindered it for the other; now we have never observed this circumstance. At the limiting distance, the sound ceased to be perceived by both listeners at the same time.

Our first trial took place the 22d June 1844, between one and two o'clock afternoon, upon a desert plain in front of the village of Saint-Cheron (Seine-et-Oise.) M. Lepileur and I successively increased our distance till it attained 254 metres; at this distance I no longer heard the diapason of M. Lepileur, and in six experiments he heard mine only once. The day was calm, the sky overcast, a very faint breeze blew from the south, that is to say almost perpendicularly to the line which joined the observers. The silence was imperfect and disturbed by the cries of birds and the buzzing of insects;

---

* Recherches sur la propagation du son dans l'air agité par le vent (*Journal de Physique*, t. lxxix. p. 285; 1814.)

† Sur l'influence que le vent exerce dans la propagation du son, sous le rapport de son intensité (*Annales de Chimie et de Physique*, t. i. p. 176; 1816.)

the temperature of the air was 24° cent., the barometer was $744^{mm}\cdot3$.

The same experiment having been repeated at eleven o'clock in the evening in the same place, it was only at a distance of 379 metres that the sound of the diapason ceased to be heard by each of us. This difference of 125 metres in the distance at which the sound ceased to be perceptible in the day and in the night, is in accordance with the consequences that Zanotti* deduced from the researches of Hauksbee; it confirms also the results obtained by De la Roche, near Paris, and the observations of M. de Humboldt upon the banks of the Orinoco,† where the cataracts were heard much better during the night than during the day, although the buzzing of insects was more intense, and the cries of wild animals were louder than during the day. M. Lepileur and I were equally surprised not to find in the night a more complete silence than in the middle of the day. The hum of the insects, the fall of the small branches from the trees, the baying of the dogs in the distance disturbed our experiment, nevertheless the sounds of the diapason were heard at a distance greater by 125 metres that at mid-day. The air was calm, the sky overcast, the thermometer at 17° cent., and the barometer at $744^{mm}\cdot7$.

The third experiment was made by M. Bravais and I, the 1st October 1844, between eleven o'clock forenoon and noon, upon the western ridge of the Faulhorn in Switzerland, at a mean height of 2620 metres above the sea. The sky was clear, the air at 7°·2 cent.; the tension of the vapour of water $5^{mm}\cdot62$; a weak breeze blew from WSW., that is to say in the direction of the two observers; the barometer stood at $558^{mm}\cdot5$. The distance at which the diapason ceased to be heard was 550 metres; on approaching each other 15 metres, we heard the diapason anew. The silence was complete. M. Bravais and I made the fourth experiment on the grand plateau of Mont Blanc, on the grand circus of snow, sheltered on the north side, situated 900 metres below the summit, and 3910 metres above the sea. At the time of the experi-

---

* *Commentarii bononienses*, t. i. p. 179; 1748.

† *Tableaux de la Nature*, t. i. p. 248.

ment, the afternoon of the 31st August, the sky was clear, the air perfectly calm, the temperature—3°·5 centigrade, the tension of the vapour of water was $0^{mm}\cdot06$, and the barometer showed $477^{mm}\cdot88$ For both of us the limit of hearing was at 337 metres.

These experiments, in which we have heard during the day the diapason at greater distances upon the mountains than in the plain, do not contradict the observations of travellers who have been struck with the weakening of sound at great altitudes. In fact, these travellers having ascended suddenly from the plain upon the mountain, their organs, and particularly that of hearing, have not had time to put themselves in equilibrium with the ambiant air. We, however, made our experiments after several days' sojourn at the Faulhorn, and upon the grand plateau of Mont Blanc, consequently our senses were, so to speak, habituated to the aërial medium. So the inhabitants of Paz and of Quito in America do not suffer from the effects of the rarifaction of the air, because they live habitually at a very great elevation above the level of the sea.

To render the hearing distances obtained on the plains and on the mountains comparable with each other, I have reduced these distances to what they would have been in the air at zero centigrade, and under the barometric pressure of 760 millimetres. In other words, I have deduced from the experiments made at different temperatures and under different pressures the limit of audibility when the air at zero has a pressure of 760 millimetres, the density of which air I designate by 1. Poisson, in his mechanics, shows what all physicists agree in admitting ; 1st, that the intensity of sound is proportional to the density of the medium on which it is produced ; 2d, that at a great distance from the origin this intensity decreases in the inverse ratio of the square of this distance. If then we make

r, the limit of audibility in air of the density *d*, as at the time of observation,

R, the limit in air of the density 1 (air at zero and 760^{mm}),

i, the intensity of disturbance of the tympanum corresponding to the limit of audibility in air of density 1,

x, the intensity in air of the density d (air of the station) at the distance R: we have, in virtue of the first law,

$$i : x :: 1 : d;$$

and from the second,

$$i : x :: R^2 : r^2;$$

whence,

$$R = \frac{r}{\sqrt{d}}$$

To calculate the distance for the limit of audibility in air of density 1, we have then to divide the distance observed by the square root of the density of the air in which the experiment was made; we obtain the density d by the following formula, in which H represents the height of the barometer, and t the temperature of the air in degrees centigrade.

$$d = \frac{H}{760\left(1 + \frac{11\,t}{3000}\right)}$$

All the distances for the limit of audibility reduced to what they would have been in air at density 1, will be found in the following table :—

Place of Observation.	Height above the Sea.	Limit of Distance observed.	Limit of Distance in air of density.	Time of Observation.	Barometer at zero.	Thermometer Centigrade.	Corresponding density of the air.
	m.	m.	m.		mm.		
Saint Chéron,	150	254	268	1844, June 22, 1h P.M.	744·3	24°·0	0·900
		379	394	2d., midnight,	744·7	17·0	0·923
Faulhorn, . .	2630	550	650	1844, Oct. 1, noon, .	558·5	7·2	0·716
Grand Plateau of Mont Blanc	3910	337	422	1844, Aug. 31, 3h P.M.	447·88	− 3·5	0·637

If we analyse these observations, we observe that there are causes which favour the hearing of sound on high mountains, that more than compensate the rarefaction of the air. We see, in fact, that we have heard a sound at the greatest

* T. ii. p. 706.

distance when the density was not more than 0·72, that at the level of the sea, as at Faulhorn; and even when the density was only 0·64 as on Mont Blanc, we have found a greater distance than on the plain.

I place *the silence* in the first rank among these causes. The summit of the Faulhorn is 900 metres above the limit of trees and of the most elevated *chalets:* the rustling of the branches agitated by the wind, the song of birds, the humming of insects, the murmuring of the brooks, and the dash of the cascades do not reach this height; hence there is a repose which is only disturbed by the noise of the wind or of the thunder. All travellers are thus struck with the silence which reigns, especially during the night. This silence is still more profound in calm weather on the grand plateau of Mont Blanc. On the Faulhorn, we still hear the grass sigh under the breath of the wind; the noise of numerous waterfalls which tumble from the flanks of the mountain may still be carried up, though feebly, by the ascending currents of air; noisy birds approach the summit; and in the heat of summer the cows and goats venture even to these heights. There is nothing similar on the grand plateau of Mont Blanc; a plain of snow surrounded by rocks elevated 3910 metres above the sea, 1850 metres above the limit of trees, and 1450 metres above the highest torrents produced by the melting glacier. The silence of death which reigns in calm weather over these fields of ice produces one of the most solemn impressions which I have experienced. This silence is so profound that sounds are heard at a great distance, although their intensity be much less than in the low country. The fall of avalanches so common in these high regions is accompanied with a noise which has no relation to the masses of the snow and ice precipitated from the neighbouring rocks: nevertheless it is always heard, because the least sound is perceived by the ear. Similarly, on the top of the Faulhorn, we hear very distinctly the avalanches which fall from the sides of the Wetterhorn; the horizontal distance of the two summits is 9700 metres, and the sound moves in a stratum of air comprised between the altitudes of 2600 and 2700 metres.

On the grand plateau M. Bravais and I also remarked a multiple echo, which repeated the human voice many times, and which was extinguished only after a duration of seven seconds. In the night of the 7th to the 8th of August we were exposed in the same place to a storm which almost carried away our tent. We were astonished at the slight noise made by the thunder claps ; the short interval between the lightning and the thunder proving to us that the concussion was not more than 1000 metres from us. Is this an effect of the rarefaction of the air, or must we seek elsewhere for the causes of this singularity ? The same night we repeated also the observation that we had already made at the rocks of the Grands-Mulets, 860 metres below the plateau, on the night of the 28–29th July, and on the grand plateau itself on the night of the 29th and 30th. During these two nights we were assailed by a strong gale from the south-west. Like Saussure on the Col du Geaut (3430^{m}),* we were struck with the terrible noise of the squalls, and with the dead calm of the short intervals of repose which separated them. These squalls succeeding the moments of profound silence produced a startling acoustic contrast, and Saussure has not exaggerated when he compares the noise of the blast to that of a discharge of artillery.

I will not enter into an examination of the other causes which in the mountains may favour the hearing at great distances. Many of them will depend on local circumstances, such as the configuration and nature of the soil, the hygrometric state of the air, the absence or the presence of aërial currents. But all these causes, the influence of which have never been studied, appear to me secondary to that which I have already signalized. Unfortunately the silence, more or less perfect, which reigns in a place cannot be expressed numerically. If that were possible, I doubt not that the intensity of the sound would be in the direct ratio of the density of the air multiplied by a quantity that we may call the *coefficient of silence*.

---

* *Voyages dans les Alpes*, §§ 2031 and 2073.

Analysis of the Waters of the Mediterranean. By M. Uziglio.

M. Marcel de Serres has made a report to the French Academy of the different memoirs by M. Uziglio, relating to the analysis of the water of the Mediterranean.

A knowledge of the composition of the water of the ocean and of inland seas, is highly interesting in a geological point of view, on account of the importance of these great fluid masses in the history of the globe. It is not less interesting to the chemist and the manufacturer, who work upon the salts which these waters contain. M. Uziglio rightly concluded that it was necessary again to analyze the water of the Mediterranean, the chemists who preceded him not having estimated with sufficient correctness the proportions of potash and soda which are held in solution.

The composition of the water of the Mediterranean cannot be compared to that of the ocean, since it is circumscribed in a basin which is closed and limited, and hence it is more concentrated. In fact, the saltness of the seas appears to have been maintained by the salts supplied from the water of the continents, and by the soluble substances which mineral waters supply in their courses. Thus, the water is generally more salt near the coast than in the open sea.* On the other hand, mineral waters, and particularly salt springs, greatly resemble sea water.

According to M. Uziglio, the principal substances contained in the Mediterranean are sulphuric, hydrochloric, hydrobromic and carbonic acids. MM. Figuier and Mialhe have also stated the presence of traces of phosphoric acid combined with magnesia. As to bases, M. Uziglio has observed potash, soda, lime, magnesia, and oxide of iron, to which for the ocean must be added oxide of manganese. The best known, and the most abundant element of sea water is chlorine ; in fact, 100 grammes of the water of the Mediterranean contain 2·0468 grs. of it, and only 0·0432 grs. of bromine, which almost constantly accompanies the chlorine ; both occur combined with the sodium and potassium.

---

* This statement requires confirmation.—*Edit.*

The most important point in the researches of M. Uziglio into the composition of the water of the Mediterranean, is the proof of the quantity of potash which it contains. According to his analysis, this quantity amounts to 0·0320 grs., or only 0·265 of potassium in 100 grammes. Notwithstanding the smallness of this quantity, M. Uziglio presumes that, before long, the potash extracted from the ocean or the Mediterranean will replace the product of the lixiviation of wood ashes, just as soda artificially extracted from sea-salt has been long and advantageously substituted for that obtained from marine plants.

When the petrifaction of shells which occurs at present in the ocean is considered, it excites no surprise that the proportion of lime is double that of the potash. In fact, 100 grammes of the water of the Mediterranean contain 0·623 grs. of lime, and the proportion contained in the ocean, according to Figuier and Mialhe, is still larger. The carbonate exists in the Mediterranean in sufficient quantity to form considerable masses of shell-limestone analogous to those of the tertiary formation, and to be substituted for that which composed the shells in their fresh state. This new calcareous matter produces also true petrifactions, analogous to those of the geological era.

One hundred grammes of the water of the Mediterranean contain 2·914 grs. of chloride of sodium, that is to say, nearly three-hundredths ; the next most abundant salt is the chloride of magnesium, of which 100 grammes contain 0·3219 grs. ; whilst the sulphate of magnesia amounts to 0·2477 grs., and the sulphate of lime to 0·1357 grs.

The large quantity of sulphate of lime which the concentration of the water of the Mediterranean precipitates in the salt marshes, would induce the belief that this salt existed in larger quantity ; and if analysis does not shew a larger proportion, it must not be forgotten that the mother waters of the salt-works are frequently renewed. On this account it will be readily conceived that, after a certain time, this salt may be deposited to a considerable extent.

Vegetables and animals contain considerable proportions of iodine, and yet the latest analyses do not indicate its existence either in the ocean or the Mediterranean. It cannot, however, be inferred that these beings have the power of forming it; it follows only that the absorbent organs of vegetables and animals are more delicate and perfect than our best methods of analysis. The quantity of bromine found in sea-water prevents the detection of iodine ; the production of the blue colour with starch may be effected or prevented, at pleasure, by repeatedly adding to a liquid an iodide or bromide ; iodine cannot therefore be detected in sea-water, while it contains bromine.

In the second memoir, M. Uziglio has examined the results of the evaporation of the water of the Mediterranean at different degrees of the areometer, and those of its analysis at different temperatures. He has given the result of his experiments on the deposits of salts comparatively with the thermometer and areometer ; those tabulated results, however useful to the manufacturer, are not susceptible of analysis.

M. Uziglio has given a table (which is capable of being advantageously extended for the use of the manufacturer) of the different saline deposits obtained at different densities. The tables which precede it shew that the progress of the continual evaporation of the water, in salt-works, is identical till the density reaches 25°, and is pretty well maintained up to 30° ; but beyond this, and when approaching 35°, the difference between day and night complicates the phenomena, so that very variable mixtures of common salt, sulphate of magnesia, and chloride of magnesium are obtained.

The results of evaporation are still more variable above 35°. The mixtures of the salts deposited undergo numerous differences in their composition, without the possibility of foretelling the result of the precipitations ; some contain from 0·5 to 0·17 of their weight of potash ; it sometimes happens that this substance is found in deposits formed in solutions, the density of which is only from 34° to 35° ; these deposits are derived from variations in the composition of the waters.—*L'Institut, Fevrier* 27, 1850, *and Phil. Mag.*, vol. xxxvi. p. 440.

On the Minerals of the Auriferous Districts of Wicklow.
By William Mallet, Esq.

The circumstances attending the original discovery of native gold in the beds of some of the streams of the county of Wicklow, have been already often detailed, and will, therefore, need but a brief repetition. The source of the auriferous streams is the mountain Croghan Kinshela, whose summit forms a portion of the boundary between the counties of Wicklow and Wexford. The stream from which most of the gold has been obtained rises on the north-east side of this mountain, and then flowing down one of the glens with which that part of the country is intersected in almost every direction, joins the Aughrim river, a little above the confluence of the latter stream with the Avonmore. It receives several smaller streams at different parts of its course, in all of which *some* gold appears to have been found, though in general in such small quantity as not to repay the cost of its extraction.

Although this part of the country, since it has been known to be auriferous, has been an object of some attraction to mineralogists, but little attention seems to have been directed to the other minerals which are to be found accompanying the gold in the alluvial deposits. These, however, are interesting, not only from their number and variety, but also from the occurrence amongst them of some of the rarer species, which do not appear to have been noticed in any other locality in Ireland. The following minerals were obtained from a considerable mass of sand and gravel taken from various parts of the bed of the principal stream:—Gold, platina, tinstone, magnetic oxide of iron, micaceous iron, red iron ochre, hydrous peroxide of iron, common clay ironstone, iron pyrites, titaniferous iron, wolfram, oxide of manganese, copper pyrites, galena, sulphuret of molybdenum, sapphire, topaz, zircon, garnet (two varieties), quartz, prase, augite, chlorite, felspar, mica.

The author has since observed, in addition to those here mentioned, arsenical iron, in small fragments, and also spinelle. The latter occurs in very small grains along with the second variety of garnet, from which it is readily distinguished by its peculiar purplish-red colour.

Gold.—This mineral occurs here in probably its most beautiful form. It possesses the true golden yellow colour and metallic lustre which characterise the metal, and, owing to the attrition to which it has been subjected, generally presents a beautifully brilliant surface. It occurs in grains of all sizes, from the smallest spangle up to a mass weighing 22 ounces, the largest hitherto found. The specific gravity of some small grains Mr Mallet found to be 16·342. The analysis of these grains gave—

Gold, ...	92·32
Silver, ...	6·17
Iron, ...	·78
	99·27

This is equivalent (neglecting the iron) to $8\frac{1}{4}$ atoms of gold and 1 of silver.

Platina.—Mixed with the gold are some very small flattened grains of a white colour and metallic lustre, which, as far as their minute size permitted an examination, appear to present all the characters of platina. They are infusible before the blowpipe, and insoluble in nitric acid, but dissolve in aqua regia. Their occurrence, intermixed with the gold when all other minerals have been washed off, is a proof of their high specific gravity.*

Tinstone.—The occurrence of this mineral in the sand is mentioned by Weaver in his reports on the gold-stream works, but he does not seem to have been at all aware of the large quantities in which it exists. From the comparatively small portion† of sand which the author had an opportunity of examining, he obtained about $3\frac{1}{2}$ pounds of stream tin; a portion of which being reduced, yielded an ingot, which, when refined by a second fusion, is hardly inferior

* It is to be wished that the existence of platina had been more fully ascertained.—*Ed. Phil. Mag.*

† The exact weight of the specimen examined, the author does not know, but thinks it certainly did not exceed 150 lbs.

to the finest grain tin.* Should this mineral be found in the mass of the sand in a quantity at all approaching that in which it existed in the specimen from which this was obtained, it would probably richly repay the labour and expense of its collection and smelting. From the small quantity in which other minerals of high specific gravity exist in the sand, and the constant supply of water, very little difficulty would be experienced in separating it from the rest of the sand; and the almost total absence of arsenic and lead would render it extremely easy to obtain from it metallic tin of the very first quality. The mineral itself occurs in grains varying in size from fine sand up to pebbles of half an inch in diameter, and in the most part of a dark brown colour, with some fragments of various tints of yellow and red; some presenting the peculiar appearance to which the name "wood tin" has been given. All these varieties are slightly translucent, some of them highly so. Many of them present distinct traces of the obtuse octohedron, the same with a short four-sided prism interposed between the two pyramids, and the latter of these with various truncations of its angles and edges. The specific gravity of some picked crystals was 6·753. A careful analysis of this tinstone gave as its constituents—

Peroxide of tin, . . .	95·26
Peroxide of iron, . . .	2·41
Silica, ...	·84
	98·51

The greater number of the minerals here enumerated are mentioned by Mr Weaver in his reports to Government on the district, and which are to be found in the Transactions of the Royal Dublin Society; but some of them, the author believes, have not been noticed before, at least he has seen no published account of the occurrence in this locality of platina, titanic iron, sulphuret of molybdenum, topaz, zircon, the small magnesian garnets, or augite. Hence it seemed interesting,

* The specimen smelted in this experiment yielded about 61 per cent. of tin; but more would be obtained on the great scale, as in this case no pains were taken to extract the tin remaining in the scoriæ.

while noticing these, to collect into a uniform, and as far as possible, complete list, all the scattered notices of the mineral wealth of this particular district, which are to be found in Mr Weaver's papers already referred to, and elsewhere.

The principal point, however, with respect to the examination of these minerals, which appears to merit further and more particular attention, is the fact of the existence of tinstone in such considerable quantity in these auriferous streams: a fact which would seem to indicate the probable existence somewhere in the surrounding district, of masses of the ore of this valuable metal of great extent, and possibly forming the continuation, on this side of the Channel, of those vast deposits which have contributed to furnish occupation and support to the inhabitants of Cornwall for more than two thousand years.*

On the Natural History of the Naked-Eyed Medusæ or Jelly Fishes.

This interesting subject, already so extensively illustrated by Professor Edward Forbes, in his beautiful work† is now engaging the attention of Professor Agassiz, who has just published "A Memoir on the Naked-eyed Medusæ of the shores of Massachusetts, in North America." Of that memoir no copy has hitherto reached this country, which we regret, as we are informed it has brought to light much that is new and important.—The species described are *Sarsia marabilis*, *Hippocrene superciliaris*, *Tiaropsis diademata*, and *Staurophora laciniata*.

Professor Silliman, on announcing the publication of this memoir, says, "Professor Agassiz, in the course of his memoir, makes some observations on the classifications of these animals, and gives reasons for including all the naked-eyed

* Transactions of the Geological Society of Dublin.

† Monograph on the British Naked-eyed Medusæ, by Edward Forbes, F.R.S., &c., Professor of Botany in King's College, London, 1848.

Medusæ in one family. He points out the little importance to be attached (except for generic and specific distinctions), to the number of tentacles, and the position of the ovaries in these Medusæ. The grand characters of the group or family are as follows:—Consisting of a gelatinous disk with the margin re-entering so as to form a cavity beneath; a central digestive cavity, from which tubes carrying chyme and forming a chymiferous system, radiate towards the margin and connect with or pass into a circular tube situated near the margin; tentacles and eye specks along this margin; mouth central, but varying in size and form; reproductive organs following the chymiferous system; a nervous ring adjoining the submarginal chymiferous tube; generation alternate, one form Polypoid, and the other Medusoid.

Among the species described, we select for particular notice the *Hippocrene superciliaris*, the observations on which embrace the principal points determined by the author. This beautiful Medusæ is a globular bell-shape animal with four bunches of tentacles on the lower margin. In the inner cavity of the bell, at centre, there is a dark four-sided mass, containing the mouth and the digestive cavity, about as broad as long. In some species, as the genus Sarsia, the mass is elongated into a moveable proboscis, in others it is scarcely projecting. From its angles, in the Hippocrene, proceed internally four tubes, which carry from the digestive cavity the chyme or fluids after digestion, diluted with more or less of the external waters: these tubes pass to the border and here connect with a circular tube which follows the margin around, and in which the chyme continues its course. This, as Professor Agassiz explains with many details, is the circulating and digestive system combined, of this and other species of the family. The tubes may be closed at their connection with the stomach, "shewing that the food is not admitted before it has undergone a certain degree of elaboration; but no sooner has it been reduced to the requisite degree of fluidity, in which the particles of the nourishing materials appear like little globules, than they open, the nutritive fluid passes into the radiating tubes, circulates regularly through these tubes along the inner walls of the disk, and through them passes into the circular tubes around the lower margin." Professor Agassiz observes, that while the circulation is properly a *chyme* circulation in these animals, it is a *chyle* circulation in Articulata and Mollusca, and in Vertebrata alone, *true blood.* The particles in the circulating fluid of these Medusæ are very irregular in size and colour, and are evidently only the imperfectly digested food.

The *nervous cord,* as distinctly made out by Professor Agassiz, follows the circular submarginal tube on its inner side, and like that, is a complete ring. At the base of the cluster of marginal tentacles there is an enlargement or bulb-like prominence. Each tentacle contains within its base a black point which subserves the purpose of vision. The bulb is not hollow in the Hippocrene, but contains an enlargement of the nervous cord, which, as our author shews, may be considered a ganglion, although not purely nervous matter in its constitution, and this position of the ganglion has a direct relation to the organs of sense clustered near it. The nervous cord, under a high magnifier, appeared as a string of several rows of nucleated ovate cells ranging in irregular lines, the cells not strictly in juxta-position by their ends, but alternate more or less so as to form a cord-like mass ; and it was distincly observed passing into the angles of the sensitive bulbs. The dark spot of the bulb consists of pigment cells, which point to the centre of minute black eye-specks, shewing a close connexion between the dark spots and the centre of the bulb where the nervous ganglion is seated ; " and though this is not an arrangement known in the organs of vision of any other animals, we are at least reminded by those peculiarities of the structure of the compound eye of insects, in which the pigment pillars intervening between the nervous mass at the base of the eyes present a structure not very different from that of the radiating cones in the bulb of the Hippocrene."

There is a similar ganglion in each of the four bulbs—a branch of the nervous thread, or what appeared to be a continuation of it, was detected along each chymiferous tube leading upward towards the digestive cavity ; .and above, near where these tubes bend towards this cavity, the cord passes around so as to form a circle, about the upper part of the chymiferous system or near the centre of the disk. There is thus a marginal, and (if there be no mistake as to its nervous character) a central circle to the nervous system; from the latter at a point half-way between two chymiferous tubes, a branch passes off and descends to the buccal or digestive mass below, and other branches go to the inner muscles. Professor Agassiz observes that there is a difference between the cord of the lower margin and the other threads, and he was not fully satisfied of the real nature of the latter. Instead of consisting of distinct cells, they are thin threads in which the cellular appearance is almost gone, excepting where they combine to form a plexus, in which some of the threads have the form of long caudate cells: they differ from muscular fibres as much as they do from the main nervous cord. No contraction was observed in them; this fact, and also their connexion with the chymiferous tubes and the sensitive bulbs below, the nature and position of the plexuses, as well as their branching to the digestive organ and to the muscles, favour the view taken. The close juxtaposition of the chymiferous system—the source of nutriment—with the nervous system and the sensitive bulbs and eye-specks, is remarked upon as a fact of much interest.

The muscular system is minutely developed in this memoir, and well brought out in the figures. The muscles consist of contractile cells rather than of fibres. Over the surface of the bell-shaped part of the disk the epithelium consists of irregular polygonal cells, of very faint outline, hardly distinguishable except by a kind of mosaic arrangement seen by means of their granular contents. On the sides and above the disk, as well as on the inner surface of the main cavity, these cells are more regular and distinct. The network of muscles, or rather lines of contractile cells beneath the epithelium, extend in two main directions; the main bundles being vertical, four in number, and alternating with the chymiferous tubes; and the others, transverse circular, in four narrow ranges, but with others smaller and less regular. The fibres are chiefly superficial, though penetrating somewhat into the gelatinous disk. In the inner cavity there are four vertical muscular bundles ranging between the chymiferous tubes, which terminate midway between the sensitive bulbs ; and also eight others much smaller, alternating by twos with these four. This same cavity has a transverse layer of contractile cells lining the whole interior, as was distinctly observed on close examination. It properly consists of four parts, extending from one chymiferous tube to another. There is still another muscular system extending between the four sensitive bulbs, within the lower margin, which is made up of a thick layer of muscular fibres, with which alternate some few radiating fibres crossing the former at right angles, and most numerous about the eye-specks.

It is now well known that these Medusæ, in one stage of their existence, have a polyp form, and those Hydroid polyps are, therefore, only imperfect Medusæ. The Sarsia, in one of its conditions, is a Coryna, and the species of Coryna pertaining to the Sarsia mirabilis (although not figured in this memoir, which treats only of these animals in their perfect state of development), is well known in the Boston harbour, and has been often collected by Professor Agassiz. The polyp of the Hippocrene is still unknown ; but we have here the suggestion, with some good reasons for admitting its correctness, that it is a *Tubularia*, a fine species of which is well known in the same harbour.

Many other points are detailed in this memoir which we have to pass by at this time.

Besides the species mentioned as described in this paper, the author briefly mentions two new species *Thaumantias* (T. diaphana and T. pilosella) ; and also a new genus near Hippocrene which he calls *Nemopsis*, in allusion to the fact, that two of the eye-specks of each cluster have a slender pedicel, instead of being sessile like the others, near the bases of the tentacles. The species was taken in Nantucket Harbour, June 1849, and is named *N. Bachei.—American Journal of Science and Art*, vol. x., No. 29, 2d Series, p. 272.

On the Ante-Columbian Discovery of America. By Dr ELTON. Communicated by Dr Traill.*

The object proposed by Dr Elton, is a summary of the knowledge we possess on the discovery of the Continent of America, by several adventurous European voyagers, anterior to the time of Columbus.

This subject, which has been for almost a century and a half well known to the students of northern history, was first made known to the rest of Europe by the publication of the *Vinlandia Antiqua* of the celebrated *Torfæus* in 1705; and most of the facts given by Dr Elton are extracted from that work. *Torfæus* proved from existing Icelandic MSS., that America was discovered, and even attempted to be colonized, by his enterprising countrymen, in the end of the tenth and beginning of the eleventh century; and the descriptions transmitted to us prove that they landed on what are now Newfoundland, Nova Scotia, Massachusetts, and Rhode Island.

The first adventurer was *Leif*, the son of *Eirik the Red*, who, in A.D. 995, when attempting to pay a visit to his father, the colonizer of Greenland, was driven by stress of weather to the coast of Newfoundland, which he named *Helluland* or *Rocky Land.* From that he sailed south-westward, till he arrived at a country, which from being covered with wood, he denominated *Markland;* and which, from the course and length of his voyage, is believed to be a part of Nova Scotia. Pursuing his course southwards, he reached a portion of Massachusetts, not far from Cape Cod; and coasting along this, he took up his winter quarters in a fertile country, which, from his description, is easily seen to have been about Rhode Island. This region, from the discovery of a species of wild vine found there, he termed *Vinland.* In the summer, he fitted out his vessel, and sailed to join his father in Greenland.

The fame of his discovery induced his brother *Thorwald*, in A.D. 1002, to sail for *Vinland*, intending to settle there;

* *Vide* Proceedings of the Royal Society of Edinburgh, No. 38, vol. ii.

but in one of his excursions he encountered and was slain by a people, the Icelanders, in contempt denominated *Skrelings*, evidently Esquimaux, who then appear to have possessed the shores far to the south of their present location.

The next and most remarkable voyage to Vinland, was that of *Thorfinn Karlsefne*, which took place in A.D. 1006. He carried with him his wife, and one hundred and thirty-one followers, and domestic animals, with the intention of establishing a colony at the huts built by Leif in Rhode Island. The soil and climate were suitable, and they remained in that country till 1011, when they were attacked by a vast number of *Skrelings*, whom they repulsed; but the hostility of the natives induced him to abandon his design, and he finally settled in Iceland. *Thorfinn*, however, had a son, *Snorro*, born in America, from whom some of the most distinguished families in Iceland are lineally descended.

After this period, it appears that there were many voyages to *Vinland*, and that Iceland sent colonies thither for more than a century; for it is stated in Icelandic MSS., that *Eirik*, bishop of Greenland, went to *Vinland* in A.D. 1125, to confirm the colonists in the Christian faith.

The work of *Torfœus* also gives us a singular account of Icelandic voyages to a country, either a continent, or a vast island, lying far to the west of the British Islands, and near Vinland. It seems to have been first visited by *Are Marson* in A.D. 983, who was driven there by a great storm. He named it *Huitramannaland*, or *Land of White Men*, from the complexion of the natives, who were also Christians; and *Are* himself was then converted from the worship of Odin to the religion of Christ.

The same land was visited afterwards by *Gudleif Gudlagson*, an Icelandic trader with Ireland; who, in a voyage from Dublin to Iceland, was driven by a tempest to a far western land, where he was taken prisoner by the natives, but delivered by their chief, who turned out to be an *Icelander*. He was dismissed with presents, but forbidden to return. The natives were *white*, and seemed of European extraction, with a dialect like that of *Ireland;* and the American archæologists, with considerable reason, have considered that their *whereabout* was on some part of the new world, between the Chesapeak and Florida.

These early voyages seem to us very surprising; but they do not seem at all foreign to the habits and enterprise of the bold Icelanders of those ages; who not only traded to every part of the west of Europe, but to the Mediterranean, and explored *Baffin's Bay*, as high as *Lancaster Sound.* We have now a certain proof, that they were at least as high in it as at 72° 55′; for in 1825, a memorial stone with a Runic inscription, and the date 1131, was found on the island of *Kingiktersoak.*

Several Runic inscriptions are said to have been found in America; but the most remarkable of these is the mass of *greywacke* on the shores of the river at *Dighton*, in the township of *Berkley*, in Massachusetts, not far from the supposed site of the settlement of *Thorfinn Karlsefne.* This has been lately carefully figured and engraved in the *Antiquitates Americanæ* of the Royal Society of Northern Antiquaries of Copenhagen, and repeated in *Jacob Aal's* translation of the *Chronicles of Snorre Sturleson.* Dr Elton, who has examined the original, assures us, that this engraving is a faithful transcript. On this rock, antiquaries read, amid figures supposed to represent Thorfinn, his wife and child, and his companions, the letters—♀*rfin* and *cxxxi*, the number of his companions.

Dr Elton next adverted to the voyage of the Welsh Prince, *Madoc*, son of the greatest of the princes of North Wales, *Owen Gwenedd*, about the year 1170. This voyage, though doubted by many, is fully believed in by Dr Elton, and it is noticed by Hakluyt, Purchas, Broughton, &c. Dr Elton quotes the singular story given by the Rev. Morgan Jones, chaplain to the British commander of the forces of Virginia, in 1669. Jones was taken prisoner by the Tusscarora Indians, who intended to torture him in their usual way, when he began to lament his cruel fate *in Welsh*, which was understood by the Indians, and he was suffered to depart in peace. These, Dr E. thinks, may have been descendants of *Madoc's followers;* and he seems inclined to ascribe to them also those very remarkable mounds, fortifications, and enclosures which are found in such quantity in the valleys of the *Mississippi* and the *Ohio*. He is inclined also to trace to these Welsh adventurers, or at least to some early Europeans, the now almost extinct tribe, the *Mandans*—a people fairer and handsomer than the *Red men*,—that are now found 1800 miles above St Louis, on the Missouri, as described by Lewis and Clarke, and Catlin, the American travellers.

These, and several other circumstances, which might have been adduced, prove that Columbus cannot be regarded as the original discoverer of the New World.

Remarks on the Snow-Line in the Himalaya. By Captain THOMAS HUTTON.

In the *Journal of the Asiatic Society*, No. 28, New Series, p. 287, for April 1849,* are some remarks on the snow-line in the Himalaya, from the pen of Lieut. R. Strachey of the Engineers, wherein he endeavours to prove that the observations some years since made by myself and others, in the northern tracts of the Western Mountains, are erroneous. (As it appears to me that this gentleman has actually left the question where he found it, I might have been induced to pass by his remarks without notice, had he not, in the excitement of an imaginary triumph, thought proper to indulge in a somewhat satirical tone of condemnation.)

That Lieut. Strachey, after three or four years of scientific researches, has at length been enabled fully to *corroborate* the previous observations of Webb and others, in Kumaon, there is no denying; but as the truth of those observations, when applied to that neighbourhood, was never called in question, there appears to have been a waste of time and ingenuity on a laborious endeavour to prove that which was already admitted to be an established fact. Webb, Hodgson, Colebroke, and the Gerards, are each and all reviewed, and in some measure found wanting, and pronounced to be ignorant alike of the true meaning of "the snow-line," and of the nature of a "glacier;" shall I, then, desire a better fate than to be condemned in the company of such distinguished observers?

Had Lieut. Strachey evinced *more* real anxiety to ascertain and establish, not a local, but the general truth, and less proneness to indulge in censure, he might have gathered from my letters in the *Calcutta Journal of Natural History*, that no attempt was made,

---

* Also in vol. xlvii. of Edinburgh New Philosophical Journal.

either by me or by those gentlemen whose opinions and observations corroborated mine, to refute the facts which Webb and others had observed in Kumaon, but that, on the contrary, while we admitted those facts to be true, we still thought we saw reason to conclude from what had been witnessed in other parts of the mountains, that they could be regarded only as locally and not generally true.

With regard, then, to the actual point in dispute, Lieut. Strachey has done nothing; for, to prove that his imaginary opponents were wrong, he would have collected his data from the districts in which their observations were made; yet, while confidently pronouncing them to be in error, he ingenuously informs us that he never was in those districts!*

What, then, is the true value of his assertions and assumptions? Does he imagine that the scientific world will be content to accept his unsupported "*ipse dixit*" in preference to the actual observations of four independent inquirers, each of whom is fully as competent as himself to judge of what he sees? Did it never occur to him, that that which may be *locally true* in one district is not necessarily true in general, when applied to the whole extent of the Himalayan range?

The first objection made to my views arises evidently from my opponent's ignorance of the localities spoken of, he, according to his own acknowledgment in a note at p. 297 of the Journal above mentioned, distinctly stating that *he never was there himself!* Yet he does not hesitate to *assume*, that "the true Himalaya," of which I wrote, was the Bissehir or southern snowy range. Had he possessed any personal knowledge of the country over which I had travelled, he would have seen that all the passes mentioned in my letters were situated *beyond* that range, and to the north of it, while, since he admits that "the mountains on which perpetual (?) snow is found all lie between the 30th and 32d degree of north latitude," a glance at his map would have shewn him that the locality of my observations is situated between 31° 30′ and 32°, or as completely beyond the Bissehir range as his own locality is north of Kumaon.

In regard to the mistakes into which I am stated to have fallen, in confounding "the north and south aspects *of the individual ridges* with the north and south aspects of the *chain*," I have to observe, that the mistake is due rather to my readers than to myself, for, in stating that "dense forests and vegetation occur along the southern *slopes*, while they are nearly altogether wanting on the northern *face*," it is evident that I referred to the true north and south aspects of *the chain;* whereas my opponents chose to imagine that I referred "to the north and south aspects of individual ridges;" hence Mr Batten's objections at page 384 of No. 19 of *Calcutta*

* Lieut. Strachey has quoted Captain Cunningham's remarks as confirmative of his own opinions, but the latter gentleman, in a recent paper, appears to plead "not guilty" to the impeachment.

Journal of Natural History, where that gentleman says, he is convinced that Captain Hutton confounds the singular with the plural number! viz., "slope with slopes." Had he been kind enough to imagine that it was just within the bounds of possibility that the final *s* was a slip of the pen, he would have been much nearer the truth. Indeed, he might have seen that such was the case, from the immediately subsequent mention of "the northern *face*," in the singular, as contrasted with "the southern *slopes*."

But although Lieut. Strachey has deemed it necessary to lay such stress upon what he imagines to be a grave error, it is remarkable that he has studiously abstained from accepting the explanation of my meaning, given at p. 380 of the *same number* of that Journal, in these words:—"Capt. Jack objects to my stating that 'dense forests and vegetation occur along the southern slopes, while they are nearly altogether wanting on the northern face;' in making this statement, I referred, *not* to the southern slopes of secondary or minor ranges on the bis-Himalayan aspect, *but* to the fact, that forests and dense vegetation are found on the *south of the principal chain or true Himalaya*, while on the northern aspect of that range they are nearly altogether wanting. This assertion will, I doubt not, be borne out by every one who has crossed into Tartary; for, while to the south of the great chain, we find superb and stately forests, on the north there is scarcely a tree to be seen, and the few that are occasionally met with are either stunted cypresses growing in the moist soil of ravines, or poplars planted round a village by the hand of man, for economical purposes."

Now, as a mathematician, my opponent should have known, that when a man assumes his own data, he ought to be able to prove anything he likes; and assuredly he is bound to establish the point for which he is contending; yet, acting on this principle, he has somehow only contrived to prove himself in error, for, knowing nothing of the western Himalaya, and *assuming* that I mean one thing, when I have distinctly stated that I mean another, he proceeds to draw conclusions which will not bear a moment's examination. Had he, before passing sentence of condemnation, bent his footsteps towards the upper parts of Kunawur, he would have found that forests *are not wanting to the north of Bissehir range*, and, consequently, that my remarks could not apply *to it* as the water-shed. It is not until the traveller surmounts the passes which lead from upper Kunawur into the Tartar districts, that he beholds on the one hand a wooded country, and on the other a comparatively barren waste, and when he has consequently placed nearly the whole of Kunawur between himself and the Bissehir range to the south.

"The doctrine," says Lieut. Strachey, "which Captain Hutton attacks as erroneous, undoubtedly is so. But it is a doctrine that was never inculcated by any one. Captain Hutton having misunderstood the true enumeration of a proposition, reproduces it according to his own mistaken views, and then destroys the phantom that he has raised." With all due deference to Lieut. Strachey, he must permit me to remind him that *assertion*, however confidently made, is neither proof nor argument, and that the doctrine to which I alluded *did exist*, may be gathered from Captain Jack's letter, in No. 15, p. 458, of the *Calcutta Journal of Natural History*, and likewise from Dr Lord's remarks or the Hindu Kush,* which, by the way, Lieut. Strachey does not deem it safe to comment upon! Moreover, "*the phantom*" which I and my supporters destroyed, was neither more nor less than this,—that whereas the common doctrine assigned *as an universal rule*, a lower elevation to the southern snow-line than to the northern, we shewed that it was only *partially*, and not universally applicable. Lieut. Strachey, however, having rejected the explanation of my meaning, as well as everything tending to militate against his own preconceived notions, and having himself misunderstood the true enunciation of *my proposition*, denies to his opponents the right of crediting the evidence of their senses, and leads them to infer that he is unwilling to admit the truth of any fact which he cannot actually see. The erroneous idea, which he has imbibed, that the Bissehir range is *my* true Himalaya, as he loves to call it, is founded on an assumption arising solely from his total want of knowledge of the localities in which my observations were made.

In quoting from Captain Cunningham's letter to me, Lieut. Strachey is careful to extract only so much as may tend to corroborate his own views; but in theorising on the probable causes which tend to accumulate a greater quantity of snow on the southern than on the northern aspect, and which, he thinks, he finds in the sudden congelation of moisture-bearing winds from the south, he is pleased altogether to disregard Captain Cunningham's observation that it is the violence of this same southerly wind which actually keeps the southern slopes of Tartary *free from snow*, and that too at all times.

Contrary to all Lieut. Strachey's views and theories, we find Captain Cunningham writing from Tartar districts that, "in January and February, and indeed *at all times*, the violent southerly winds kept southern exposures *free from snow*." Again, he says, "no snow whatever on *southern slopes* within 15 to 16,000, but *on northern slopes* and in hollows, *abundance of snow*." Again, "February 10th and 11th, in getting up *the northern slopes*, the snow was I don't know how deep; on reaching the summit of a pass, I found no snow, nor did I find any on the southern slopes, except in hollow portions or tolerably flat bits. The highest pass on the road is perhaps 13,500, or nearly 14,000 feet." (This too, be it remembered, in notoriously the severest month of winter, in these hills!) "The effect," he continues, "is attributable partly to the violent southerly

* Cal. J. Nat. Hist., No. 14, p. 276.

winds which flow during December, January, and February, and partly to the sun's rays. In the beginning of May, in coming from Nako to Chungo in Hungrung, I found no snow on the *southern*, *eastern*, or *western slopes;* but on some *northern ones*, which were steep, there was *snow three and four feet thick;* elevation about 11,500 feet. At Shalkur, up to the middle of June, the snow lay *on the northern sides* of the gullies or ravines of the hills; and when out shooting, I had much difficulty in crossing them; elevation 11,000 to 11,500 feet. I was informed also that *the northern slopes* of the Hungrung ghat, between Soongnum and Hungo, had some snow until the middle of June. *On the southern face* it had melted *six weeks before*, except in hollow places;" and, finally, "August 7th, there is no snow on western slopes of hills 17,000 feet high, *but there are a few patches on the northern slopes*."

Thus we have observations made in Tartar regions north of the Bissehir range, between 31° 30′ and 32° north latitude, all of which tend directly to prove that, while from December to August, snow was always to be found on the northern aspect of every hill or range, there was either little or none at all on their southern exposure.

What, then, has Lieut. Strachey proved by his observations in Kumaon, and by his strictures upon nearly every one who has written on the subject of the snow-line in the Himalaya? We appear to be indebted to him simply for proving what was never disputed, namely, that the facts observed by Webb and others in Kumaon are true, as far as regards that district; but with respect to the only point in dispute, namely, as to whether those facts are only locally and not generally true, he has left the question exactly where he found it.

But conceding even that the Bissehir or southern snowy range was the locality on which my facts were observed, there still appears strong reason for asserting that the phenomena there visible are directly opposed to the conclusions which my opponent would draw from them; for he declares that a greater quantity of snow must fall on the outer southern face of the range, owing to the interception of heated and moisture-bearing winds from the south, and thus he would account for the prevalence of the snow on that aspect. Supposing then, for the sake of argument, that thus far his views are just, when applied to the southern range of Kumaon, he has still chosen to overlook the fact, that in Loyd and Gerard's "Tours in the Himalaya,"—a work, too, which he has himself quoted,—it is stated that "the line (of perpetual snow) in the latitude 30° 30′ in Asia, is fixable at 15,000 feet on the southern or Indian aspect of the Himalaya mountains, and on the northern (not the Tartaric) may be concluded at 14,500 feet." This appears to me to give the northern snow-line of the outer range an elevation less by 500 feet than the southern one; while Captain Cunningham, in a recent paper, even estimates the approximate difference at 3,000 feet.* The same gentleman likewise states that "on the Tibetan side of the chain, the (approximate) heights will be found to be 20,000 feet on the south, and 18,000 to 18,500 on the north face of the same hill." These observations then appear to establish the fact, that from the southern snowy range to the northern or Tibetan one, the snow-line is always, on every hill or range, the outer ones inclusive, at a lower elevation on the northern than on the southern *slopes*.

But Gerard proceeds to tell us, that "the cheeks (of the Borendo pass, on the Bissehir range) are perfectly naked long before this time of the year (August 1822), and the trough formed by them, although sheeted with snow at the summer solstice, is now (August) bare rock down to the ravine *on the south side*, with the exception of some accumulations, which will be very much diminished before another month; and some seasons, as in the former (1821) *the whole face of the declivity without a patch of snow.* On the north *there lies a vast field* which never dissolves."†

So again, Captain Jack says, "I crossed the Borendo ghat on the 25th September 1842, and *there was no snow at all on the southern aspect*, or on the very summit of the pass; but descending a few yards *on the northern aspect* to the base of a rock which was nearly perpendicular, we had the pleasure of seeing our baggage, coolies, &c., descending most rapidly, by their own gravity, upon an unbroken bed of snow, extending 250 to 300 *yards in one slope*, forming an angle of about 45°."

Here, then, we have different observers in different years, proving that on the Bissehir range the snow lies deeply and extensively *on its northern face*, even when there is *none on its southern aspect;* we have, consequently, the very same phenomena apparent, from the outer snowy range up to the northernmost one, proving that the local facts of Kumaon are *not facts* in the western parts of the Himalaya, and showing moreover, since the true southern aspect of *the chain* becomes denuded of snow, that while there is a snow-line on the northern or Tibetan aspect, there is no permanent snow-line on the southern face of the Bissehir range.

It is however due to my opponent to state, that I am not aware that the elevation of the passes on the Bissehir range have ever been correctly ascertained; for although Dr Gerard has somewhere stated the cheeks of the Borendo pass to be upwards of 16,000 feet, yet the truth of that measurement has been since called in question. It may therefore eventually be found, that the elevation of that pass is below the snow-line, which would account for the disappear-

---

* J. A. S., No. 205, p. 695, for 1849, and Jameson's Edinburgh New Philosophical Journal, vol. xlviii. p. 243.

† Loyd and Gerard's Tours in the Himalaya, p. 327.

ance of the snow from the southern aspect. I am quite willing, then, to give Lieut. Strachey the benefit of the doubt; while, at the same time, should I be driven from my first position in Bissehir, I shall take my stand with Dr Lord on the Hindu Cush, and maintain (which is in fact the only point for which I have really contended), that the doctrine on which Humboldt relied, as applicable to the whole extent of the Himalaya, cannot be so accepted. Feeling satisfied that he had discomfited all former observers in India, and thus converted his local into general facts, Lieut. Strachey next proceeds to run a tilt with Humboldt himself, who had accounted for the greater elevation of the snow-line on the north of Kumaon, by supposing that the radiation of heat from the plains of Tibet contributed mainly to produce that effect. With this very simple and natural inference our author is dissatisfied, and he "therefore attempts to supplant it with a theory of his own. He says, that as radiation from the plains of Tibet *does not* produce the greater elevation of the northern snow-line, that effect must be occasioned by the diminished quantity of snow that falls on the northern, as compared to the southern part of the chain." Now this, if it be intended to apply likewise to the district of Bissehir, becomes a perfect riddle; for, if less snow falls on the north than on the south, how is it that there is always snow on the northern long after it has disappeared from the southern aspects of the higher ranges of the western tracts? Are we to believe that the greater the quantity the sooner it melts?

Even if restricted to the neighbourhood of Kumaon, the theory would be totally unsatisfactory, for the small quantity of snow on the north, if not acted on by radiation of heat from the plains of Tibet, nor melted by the rains of the monsoon, would last, at the very least, as long as double the quantity on the northern slope, where it is exposed both to the direct rays of the sun and to the destructive influence of the heavy periodical rains; and this appears to be very satisfactorily proved by Lieut. Strachey's own remarks on the black range, which, rising immediately from the plains of Tibet, retains snow on its northern aspect when there is none whatever on the south. But when to the effects of the above agents we add the fact, that the violent southerly winds of winter have a tendency to keep the southern slopes free from snow, and to accumulate it in drift on the north, we appear to have every fact leading to the conclusion, that the snow will, as a general rule, be found longer and deeper on the north than on the south; and Captain Cunningham has stated, that when (even in winter) there was little or no snow on southern aspects, it was sometimes "*four feet thick*" on the north!

The very admission, therefore, that the northern destructive agents exert little influence on the snow, would of itself be sufficient to overthrow thus much of Lieut. Strachey's theory; for if those agents which drive the snow to a certain elevation are removed, it is evident that the snow, whether much or little, must remain nearly or altogether intact.

We are further told, that "the air that comes up from the south no sooner reaches the southern boundary of the left of perpetual snow, where the mountains suddenly rise from an average of perhaps 8,000 or 10,000 feet, to nearly 19,000 or 20,000, than it is deprived of a very large proportion of its moisture, which is converted into cloud, rains, or snow, according to circumstances. And the current, in its progress to the north, will be incapable of carrying with it more moisture than is allowed by the very low temperature to which the air is of necessity reduced in surmounting the snowy barrier, 19,000 or 20,000 feet in altitude, that it has to pass. Nor can any further condensation be expected at all comparable in amount to what has already taken place, as it would manifestly demand a much more than corresponding depression of temperature; and this is not at all likely to occur, for the most elevated peaks being situated near the southern limit of perpetual snow, the current on passing them will more probably meet with hotter than with colder air."

I must confess that this theory does not appear to me to be either conclusive or even probable; for, in the first place, we are neither furnished with any *proof* that the air will be *hotter* to the *north* of the high peaks, nor with any approach to data for determining the question; the whole resting upon the unauthorised *assumption* of a *desired fact*, the existence of which is absolutely necessary to give anything like validity to the theory.

Were the upward or northward passage of the moist air effected slowly and gently, no doubt we might expect a heavier fall of snow on the southern aspect of the chain, *provided always* the temperature beyond it was, as Lieut. Strachey supposes, *hotter* than on the Indian side; but *this is not the case*, as is most convincingly proved by the admission that snow always lies longer on the northern aspects *of all hills and ranges* than on the south, and I need only cite Lieut. Strachey's own black range as an instance of the fact. He likewise admits that "southerly winds blow throughout the year over the Himalaya;" and "in the winter," which is of course the season of snow, "*with the peculiar violence.*" This is recorded also by Gerard and by Captain Cunningham, and every traveller can confirm the same. But this very violence of the southern winds must necessarily carry the snow across the southern range, and accumulate it deeply to the north; and this is clearly shown to be the case by Captain Cunningham, who relates that, while during winter, and "indeed at all times, the violent southerly winds kept southern exposures *free from snow*,"—"*on the north it was I don't know how deep.*" Moreover, if the temperature of the air was hotter to the north than to the south of the high peaks, we ought, as we approach the plains of Tibet, to find no snow on the northernmost range; yet the black range, rising from those plains, retains the snow on the northern, even when there is none on the southern slope,—a fact which, while it militates strongly against Lieut. Strachey's views, tends much to corroborate Captain Cunningham's observations. But granting that Lieut. Strachey were correct in these particulars, does it necessarily follow, that what is fact in the neighbourhood of Kumaon may not be pure fiction when applied to the western tracts? Can the assumptions of one who confesses that he never set foot within the limits of the district where his opponent's observations were made, in any way affect those observations? He is evidently disposed to disregard the question of one of his own supporters, who asks,—"How can any facts of one observer in one place falsify the facts of another observer in another place?" * Now I and my supporters have long since received Captain Webb's Kumaon facts as *true*, when applied to the places wherein he observed them, and we merely, in return, claim the right of believing the evidence of our own senses, when wandering over other tracts of the Himalaya.

I repeat, then, that, as far as the evidence yet goes, the phenomena observable in Kumaon are opposed to those which have been observed to the westward; and in rejecting Lieut. Strachey's theory as insufficient, I much prefer adhering to Humboldt's until a better is offered. Lieut. Strachey denies that the radiation of heat from the plains of Tibet exercises any but a trifling influence on the snows of the northern aspect; still his denial rests on no better basis than that of an assumption, for no proof whatever is produced in support of the opinion, save that there is snow on the Tibetan face of the black range, when there is none on the southern face. But this is really nothing to the purpose, for it merely shows that the direct rays of the southern sun, united to the greater humidity of the atmosphere, and the effects of the violent southerly winds, have a far more powerful effect in uncovering the southern aspect than the heat from the plains of Tibet has upon the snow of the north. The true question, however, does not relate to the north and south aspect of the black range, but to the aspects of the water-shed; and in regard to it we are told, that, while on the south the snow-line is about 15,000 or 15,500 feet, on the north it is 18,000 to 19,000 feet. Now the height of the northern ranges above the plains of Tibet does not appear, on an average, to be more than 3,000 to 8,000 feet, if so much; while on the south, the peaks rise to 16,000 and 18,000 feet above the plains of India, from which, moreover, they are separated by a broad intervening belt of wooded mountains, averaging from 6,000 to 8,000 feet above those plains. Consequently it does not appear very difficult to perceive that radiation from the northern plains must affect the snow

* Cal. Journ. Nat. Hist., No. 19, p. 383.

more powerfully than from the southern plains, and will drive the snow-line to a greater elevation *above the sea* on the northern than on the southern aspect. Thus Humboldt's theory, when applied to the Kumaon and other similar districts, appears to be perfectly correct. But that the physical features of the Kumaon and western tracts are at the antipodes of each other, has been plainly stated by Mr Batten, who says—" Our passes *at once* take us into Tibet, and do not conduct us, like those beyond Simlah, into an intermediate and peculiar tract, like Kunawur." * Now it seems to me by no means improbable that this very difference in the features of the two tracts may be sufficient to account for the difference in the phenomena observable in each, and that if Humboldt's theory of radiation from the plains of Tibet is sufficient to account for the retreat of the snow to the heights of the northern face, the want of similar plains† in the western tracts will of course preclude such radiation from acting on the northern face of the western mountains, and thus the greater heat of the southern side, added to the periodical rains and to the violence of the winds in winter, will leave snow on the northern long after it has disappeared from the southern aspect.

Lieut. Strachey admits that the rains have a powerful effect in melting the snows; but his want of knowledge of the localities to the westward has led him into an error, when he supposes that the monsoon does " not extend up the Sutlej beyond the point where the Burpa falls into it;" the truth being that Chini, which is itself farther up, and situated in the gorge where the Sutlej breaks through the outer snowy range, is full within the monsoon, as both Captain Jack and I experienced; beyond this point the rains are light and uncertain, but they nevertheless extend to the head of the district, for clouds and vapours pass onwards through the valley of the Sutlej, even to the upper parts of Kunawur, and exercise great influence on clearing the southern slopes of their snow; and although Lieut. Strachey has *assumed* that clouds protect the snow, by warding off the direct rays of the sun, he overlooks the fact that such clouds betoken a humid atmosphere, which is quite as inimical to the duration of the snow as the sun's rays, and he might, at least during his scientific researches in Kumaon, have learnt the fact that thaws are more rapid in cloudy weather than in a dry and unclouded atmosphere, such as that which he acknowledges to be the general characteristic of the northern aspect.

Dr Lord's remarks on the Hindu Cush coincide apparently with mine to the north of the Bissehir range; and since Webb's observations in Kumaon are found to be only locally true, there can be little doubt that Dr Lord's surmise relative to the effect of heat

* Cal. Jour. Nat. Hist., No. 19.

† Captain Cunningham seems to doubt the existence of any plains at all.—*Vide* J. A. S., No. 205, for 1849.

radiating from the high plains of Cabul and Koh-i-Damun is correct.

Thus Lieut. Strachey's observations, although useful in corroborating those of Webb and others, in reality leave the question precisely where it was, namely, that while in Kumaon the elevation of the snow-line is greater on the northern aspect than on the southern; the truth, on the Hindu Cush, and, as far as observation goes, in the Tartar districts north of the Bissehir range, is actually the reverse; proving, as I long since stated, and now repeat, that the facts on which Humboldt relied as applicable to the whole extent of the Himalaya, are found to be purely local, and dependent altogether on the physical features of the country to the north and south of the water-shed.*—*Journal of the Asiatic Society of Bengal, New Series*, No. xxxiii. p. 954.

On a Remarkable Compound of Iodine and Codeine. By THOMAS ANDERSON, M.D., F.R.S.E., Lecturer on Chemistry, Edinburgh. Communicated by the Author.

In a paper on the constitution of codeine, and its products of decomposition, published in the last part of the Transactions of the Royal Society of Edinburgh, I mentioned the existence of a compound of iodine and codeine which I was at the time prevented from describing by difficulties encountered in the analysis. These difficulties which were experienced in the determination of the iodine, I was at length enabled to overcome by a slight modification of the process usually employed for determining that substance, and I am now in a condition to describe the compound to which I give the name of Teriodide of Codeine.

The action of iodine on the vegetable alkalies and among others on codeine, has been already examined by Pelletier,† but from the account of his experiments on this base, which were obviously very cursory, he does not appear to have obtained any definite, and certainly no crystallisable compound. He mentions that iodine and codeine combine together directly, forming a brown compound sparingly soluble in water, and

---

* A similar explanation of the various heights of the snow-line in the Pyrenees is given by Rozet.—*Edit. Edin. New Phil. Journal.*

† Annals de Chimie et de Physique, vol. lxiii. p. 164.

that the same substance is also formed by treating codeine with an alcoholic solution of iodine, but that it is then accompanied by much hydriodate of codeine.

The teriodide of codeine is prepared by dissolving equal weights of iodine and codeine in as small quantities of alcohol as possible, mixing the solutions and leaving the mixture to itself. In the course of a period varying with the degree of concentration of the solution, the new compound is deposited in crystals. If saturated cold solutions have been employed, they begin to appear in the course of a few hours, but if dilute, a longer period is required. These crystals are generally very small, but on one occasion I was fortunate enough to obtain them of sufficient size for measurement, and the numbers obtained will be found in a paper by Professor Haidinger of Vienna, appended to the present notice.

Teriodide of codeine is deposited from its solutions in small triangular plates of a fine ruby red colour, by transmitted and deep violet by reflected light; these crystals are extremely brilliant, with a fine adamantine, and in strong lights almost metallic lustre. It is insoluble in water, but dissolves in alcohol with a reddish-brown colour, and the saturated hot solution deposits crystals on cooling. It is insoluble in ether. Concentrated sulphuric acid has no action on it in the cold, but on the application of heat it is dissolved with a deep brown colour. It is slowly attacked by hot nitric acid. Treated with a boiling solution of potash it is decomposed, iodine being dissolved and codeine left behind. A current of sulphuretted hydrogen passed through its solution rapidly decolorises it, sulphur is deposited, the solution becomes highly acid, and on evaporation deposits colourless crystals of hydriodrate of codeine. A solution of nitrate of silver, even in the cold, decomposes it instantaneously with the formation if iodide of silver.

As it is apt to lose iodine at 212°, the quantity employed for analysis was dried in vacuo. The following are the results obtained:—

I.	7·725	grains of teriodide of codeine gave	
	9·020	...	carbonic acid; and
	2·283	...	water.

II. $\begin{cases} 7{\cdot}906 \text{ grains of teriodide of codeine gave} \\ 9{\cdot}352 \quad \ldots \quad \text{carbonic acid; and} \\ 2{\cdot}448 \quad \ldots \quad \text{of water.} \end{cases}$

III. $\begin{cases} 8{\cdot}621 \text{ grains of teriodide of codeine gave} \\ 10{\cdot}216 \quad \ldots \quad \text{carbonic acid; and} \\ 2{\cdot}722 \quad \ldots \quad \text{of water.} \end{cases}$

IV. $\begin{cases} 8{\cdot}042 \text{ grains of teriodide of codeine gave} \\ 9{\cdot}245 \quad \ldots \quad \text{carbonic acid; and} \\ 2{\cdot}413 \quad \ldots \quad \text{water.} \end{cases}$

The determination of the iodine was at first attempted in the ordinary way by combustion with quicklime. In repeated experiments, however, numbers were obtained varying from 49 up to 53, and on one occasion nearly to 54 per cent.; but, notwithstanding every precaution in making the experiment, it was found impossible, by this method, to obtain the full amount of iodine. The substitution of carbonate of potash for lime was attended with much more satisfactory results. As carbonate of potash cannot easily be obtained absolutely free from chlorine, I employed a specimen of that substance containing a known amount of chlorine. A fixed weight of this carbonate was mixed with the material for analysis, and the combustion conducted in the usual way, a high temperature being sustained during the whole experiment. After combustion, the contents of the tube were dissolved in water, and *dilute* nitric acid slowly dropped in, until the solution was nearly but not entirely saturated. Nitrate of silver was then added, and after it a quantity of nitric acid sufficient to render the fluid distinctly acid. The precipitate was washed in the usual way and weighed, and then by subtracting from it the amount of chloride of silver corresponding to the chlorine which was present in the carbonate of potash employed, we have the weight of the iodide of silver. The first trials made by this method gave a considerable excess; but this was traced to a quantity of carbonate of silver having been carried down with the iodide, and protected by it from the action of the nitric acid with which the fluid was afterwards super-saturated. All that is necessary to avoid this source of error is to add a few drops of nitric acid to the water with which the iodide is washed, and to digest it for some time at a moderate temperature, after each addition of water and acid.

V. $\begin{cases} \text{5·255 grains of teriodide of codeine gave} \\ \text{5·590 ... ... iodide of silver.} \end{cases}$

	I.	II.	III.	IV.	V.
Carbon, . .	31·84	32·20	32·30	31·96	...
Hydrogen, .	3·28	3·44	3·50	3·33	...
Iodine, . . .	...	...	...	...	55·32

These results correspond closely with the formula $C_{36}H_{21}NO_6+I_3$, of which the following are the calculated results compared with the mean of analysis :—

	Mean.	Calculation.		
Carbon, . . .	32·07	31·75	C_{36}	216
Hydrogen, . .	3·38	3·08	H_{21}	21
Nitrogen, . .	...	2·05	N	14
Oxygen, . . .	...	7·12	O_6	48
Iodine, . . .	55·30	56·00	I_3	381·3
		10·000		680·3

The relation of teriodide of codeine to nitrate of silver is of a very remarkable kind. It is instantaneously decomposed by solution of the nitrate, and this reaction appeared at first sight likely to afford the most accurate and commodious method of determining the iodine, a method which has already been applied by Pelletier to the analysis of the compound of iodine and strichnine. Experiment, however, proved it to be entirely inapplicable, as the whole of the iodine is not obtained in the state of iodide of silver, but always a much less amount, which also appears to be constant, although not bearing any satisfactory atomic relation to the whole quantity. Three different determinations of the iodine were made by this method with the following results :—

		Per cent.
5·590 grs. teriodide of codeine gave	4·45 grs. iodide of silver	= 43·01
5·304	4·24	= 43·19
7·449	5·795	= 42·04

If we suppose three equivalents of teriodide of codeine to yield seven equivalents of iodide of silver, we ought to obtain 43·6 per cent. of iodine in this state. Assuming this to be the case, which, however, has not much probability in its favour, two equivalents of iodine remain, which must have passed into some other state of combination. The quantity of material at my disposal was too small to admit of detailed experiments to determine this point; and, after all, the close approximation in two of the determinations of iodine may be merely fortuitous.

I shall not attempt to enter here upon the question of the exact constitution of the teriodide of codeine, which cannot be satisfactorily deduced from the examination of an isolated substance. In order to do so we should require a complete investigation of the analogous compounds of the other bases, of which at present we know almost nothing. Many of these compounds are exceedingly beautiful substances, and have been partly examined by Pelletier and others, although not in a satisfactory manner, and all of them both require and deserve reinvestigation.

Barrande on the Silurian System of Bohemia. Communicated by JAMES NICOL, F.R.S.E., Professor of Geology, Queen's College, Cork.

A general account of the structure of Bohemia from the pen of Sir R. Murchison, who had shortly before visited that country, and examined its more interesting localities, formerly appeared in this Journal.* That distinguished geologist has kindly communicated to us some portions of a work now in course of publication, by M. Joachim Barrande, already so well-known from his "Preliminary Notice of the Silurian System of Bohemia," and his descriptions of the Brachiopods of that country. The present work, now publishing by subscription, is intended to form three quarto volumes, and will be illustrated by many plates of the organic remains. Some of these, chiefly of the trilobites, which occur in such abundance and fine state of preservation in Bohemia, are now before us, and are very beautifully executed. Any general account of the work must, however, be deferred, till it is more nearly completed, and the following notices of some of the

---

* See Jameson's Edinburgh New Philosophical Journal, for January 1848.

more important results already obtained by M. Barrande, are chiefly supplementary to Sir R. Murchison's memoir above mentioned.

Bohemia, as is well known, forms a great basin or mountain valley, shut in on every side by walls of ancient granitic and gneiss rocks, except on the north and north-east, where the new red sandstone and more recent formations spread over the older rocks. The longer axis of this basin runs north-east to south-west, from a little south of Prague to Bischof-Teinitz, and is about 20 German (92 English) miles long. The whole interior of this great valley is filled with deposits, belonging to the same period with the Silurian formations of England. These deposits, in general, dip from both sides towards the central axis of the valley, and thus form a synclinal trough or basin. The dip of the strata is commonly from 30° to 45°, rising occasionally to 70°, or even becoming vertical.

The whole mass of these rocks occurs in conformable stratification, but may be divided from palæontological and petrographical characters into eight stages (étages) of which four correspond to the lower, and four to the upper division of the Silurian system of Sir R. Murchison. These are considered by the author in the ascending order.

The lower division is by far the most extended, both in superficial and vertical dimensions. Resting immediately on the granite are found talcose, chloritic, and other schists, which in some localities, as at Zampach, seem to alternate with it. These schists pass insensibly above, into primary clay-slate (*Urthonschiefer*), and this as we continue to ascend into schistose conglomerates, composed sometimes of fine quartz grains, at other times of larger masses, more or less rolled, and united by an argillaceous or siliceous basis. These rolled masses, often less than nuts, rarely so large as a man's fist, consist of quartz of various colours, of clay-slate and flinty slate. The whole of these deposits are without organic remains, but from their conformability to the superior beds and gradual passage into them, are considered by the author as the azoic base of the Silurian formation. He divides them into two groups, the lower, A, comprising the crystalline and semi-crystalline rocks mentioned above; the higher, B, the clay-slate. This rock is sometimes the fine grained clay-slate, with alternations of flinty slate; at other times it becomes more or less of a conglomerate. True conglomerates also occur both in stage B, and in the two next higher stages, C and D. They abound especially in the south-east of the basin, almost to the exclusion of other rocks, and thence run out along the central axis of the valley. Their vertical thickness is also greatest on the south-west, rapidly diminishing to the north-east, which is likewise true of the whole deposit. The size of the fragments too, decreases in the same direction, so that it may even be said that in the north-east third of the basin there are no conglomerates, but only very fine siliceous sandstones or quartzites. From these facts the author concludes that this portion of the formation has been deposited by currents running from south-west, parallel to the axis of the basin, and debouching on the north-east into the open Silurian sea. His facts seem rather to indicate a delta deposit in a great gulf, the materials of the conglomerates being also found in the ancient rocks on the south-west. Limestone only occurs in this part of the series in veins, or in very rare and insignificant lenticular masses. In the stage B, the two most important metalliferous districts of Bohemia occur. The veins of argentiferous lead-ore, wrought at Przibram, near the middle of the south-east side of the basin, traversing the clay-slates and fine grained conglomerates or greywackes. The mining district of Mies, on the north-west side of the basin, is also in fine clay-slates intercalated with flinty slate.

The next higher stage, C, is that of the protozoic schists, distinguished from the inferior group principally by containing organic remains which have not been observed lower. They are fine grained argillaceous slates, with a small proportion of silica and oxide of iron, and very minute scales of mica. Their colour is often green, like the Silurian rocks of England, but becomes brown on decomposition, when some of them also separate into spheroidal crusts. This group is only found in two detached portions, one on the NW. side of the axis near Skrey, the other on the SE. at Ginetz, but the author considers these as at least identical in age, and perhaps even connected below the newer beds. They both seem to thin out towards the NE. extremity of the basin, and are only well seen in a few deep river gorges. Thus the band of Ginetz, though from 300 to 400 metres thick, is yet intercalated between such enormous masses of quartzites and conglomerates, as almost to disappear. In consequence of the degradation of the surface, the soft slates have formed a longitudinal valley between high and steep escarpements of the harder masses, and these at length yielding to the influence of the atmosphere, have covered up the slates with a great thickness of siliceous ruins. When to this is added the vegetable soil formed under the shade of the primeval forests that still flourish on the mountains of the Trzemoschna and Brdi-Wald, it will be easily seen that the labour of the geologist, in tracing out these deposits, is by no means light.

These beds contain a distinct group of organic remains, which M. Barrande regards as the primordial fauna of Bohemia. This fauna contains Trilobites in abundance, some forms of the family of Cystideæ, and a few species of Orthis. The other classes are not represented. Now, among the Trilobites, the dominant forms of that early creation, it is remarkable, that not only all the species, but all the generic types except the genus *Agnostus*, belong to it exclusively, and do not live on into the higher stages. This is true also of the species of the Cystideæ and Orthis found in stage C, which thus exhibits almost no connection with the other Silurian formations. Hence some event must have caused either the sudden extinction or the emigration of the myriads of Crustaceæ which swarmed in this sea. In the NW. band, masses of porphyry are seen in the vicinity of Skrey, alternating with the upper part of the formation, and changing the character of the rocks. This eruption of igneous matter spreading out over the bottom of the sea, may not only have destroyed life there, but the deleterious influence of the substances thrown into the water may have produced the same effect on the south-eastern band.

The two bands contain twenty-five species of Trilobites, of which five are common, ranged under seven genera. One genus, the *Paradoxides*, Brong., is also found in England and Sweden, but no identical species have yet been observed. The *Agnostus* is also common to all the three countries ; and the *Conocephalites* of Bohemia seems to represent the *Olenus* of Sweden and England. In the latter country some forms in the oldest fossiliferous rocks show a strong analogy to those of Bohemia. These are the few relations connecting this Bohemian fauna with that of other lands, the genus *Agnostus* being alone common to it and the Silurian rocks of Russia, France, or the United States of America.

Next in the ascending order is the stage of the quartzites, D, so named from the predominance of a rock of this nature. This deposit forms a long ellipse, extending from Rokitzan almost to the Elbe, a distance of fifty English miles, with a maximum breadth of eleven miles. It rests partly on the former stage C, partly towards the SW. on the old azoic rocks B, and is covered in the centre by the newer rocks which it encloses as in a ring. Its vertical thickness is very great. The lower parts consists predominantly of siliceous rocks,—flinty slate, especially above the porphyries in the NW. band of C ; siliceous conglomerates, particularly on the SE. side of the axis ; and quartzites, sometimes composed of distinct grains of quartz, cemented either by pure silica or this mixed with clay, at other times presenting so fine a texture and such a brilliant vitreous aspect as to resemble rather a chemical than a mechanical deposit. The beds vary in thickness from a few centimetres to two metres, and are divided by thin argillaceous or schistose layers, which seem to mark periods of intermittance in the deposition of the siliceous rocks. These interruptions have been sufficiently long to divide the whole stage into concentric bands, running the whole length of the basin, but, like the conglomerates, thicker on the south-west. The alternating schistose rocks form five principal masses, distinguished by peculiar characters. The upper three, near the top of the stage, are named black laminated slates (*Schistes noirs feuilletés*), very micaceous slates (*Schistes tres micacés*), and yellowish grey slates (*Schistes gris jaunâtres*), and are all collected near the top of the stage in one mass, seldom under 1000 metres thick, and containing only thin bands of quartzite. They all yield readily to the action of the atmosphere, and the powerful erosion of the surface has produced a valley of an elongated annular form, corresponding to the outline of the mass. This valley is bounded on the north-west side by the thick band of quartzites forming the Drabow mountains, and on the other side of the axis by the corresponding mass of quartzite in the Brdi-Wald. From the middle of this valley the more recent calcareous groups rise up like an island to the height of 100 metres or more.

In this, as well as in the inferior stages, limestone rocks are almost entirely wanting,—a circumstance characteristic of the lower division of the Silurians of Bohemia, and perhaps explanatory of the diversity of its fauna from that of other similar regions. But the slates above mentioned, near Beraun and other localities, contain spheroidal masses, in some of which a large proportion of carbonate of lime is found, whilst in others calcareous matter is entirely wanting.

Two other very remarkable deposits of calcareous spheroids occur in the micaceous slates, about six kilometres (four miles) to the south-west of Prague, one on each side of the axis. The more northerly at Mottol, near the road from Prague to Beraun, is accompanied by a mass of trap in its entire length. The other lies near Grosskuchel on the Moldau. Both contain fossils similar to those in the higher division, and are embedded in a mass of slates with graptolites, exactly like those forming its base.

Though all the species, and even the generic types, with the exception of *Agnostus* and *Orthis*, had disappeared at the close of the former stage, there is still a certain order and connection in the successive development of animal life in this isolated region. The Trilobites continue to predominate, both from the multiplicity of their forms and the number of the individuals. The new genera and species are introduced, not all at once, but at intervals, and as it were in a fixed relation to the changes in the medium in which they lived, as marked by the successive changes in the mineral character of the deposits. Towards the close of the period, some representatives of the family of Cytherinides also add to the richness of the Crustaceans. Among the Mollusca, a few rare fragments of Orthoceratites, from the quartzites of the Drabow mountains, mark the first appearance of the Cephalapods; and with them are associated the Pteropods, in several species of *Conularia*, soon followed by the *Pugiunculus* in the black schist. The same quartzites present the first traces of the Heteropods in some forms of Bellerophon, and also of the Acephalæ, in a few species of *Avicula* and *Nucula*, whilst the Gasteropods appear in the micaceous slates in the *Pleurotomaria*, and other forms analogous to the *Holopea* of Hall. The Brachiopods are represented in each of the fossiliferous bands by peculiar species of *Orthis*, whilst, in the micaceous slates, the genera *Orbicula*, *Lingula*, *Spirifer*, *Leptæna*, and *Terebratula* also appear. The great class of the Echinoderms is represented by some considerable traces of Crinoids, by the Cystideæ, forming almost entire beds, one or two metres thick, in some localities near the middle of the micaceous slates, and, in the same formation, by some very rare *Asteriades*, together with a body very analogous to that which Professor E. Forbes has recently described as the *Protaster Sedgwickii.* The *Agelacrinites* dates from the Drabow quartzites. Polypariæ are extremely rare in this stage, the *Calamopora fibrosa* being the first that occurs in the micaceous slates, followed, but only in the calcareous nodules, by *Calam. Gothlandica.* In the same deposits Graptolites also appear, but have never been observed lower. On the whole, with the exception of the Trilobites and Cystideæ, which seem to have swarmed at several epochs, all the other classes of animal life were but feebly represented in those seas. The vegetable kingdom, too, is only indicated by very indistinct traces of Fucoids in all the formations of this stage.

These facts show that life was now more abundant than in the lower stage. The Trilobites shew this very remarkably. In C, 7 genera with 25 species were known; in D, there are 19 genera with 49 species, or 12 genera and 24 species more in the upper than the lower group. Nine of the genera, including *Asaphus* and *Trinucleus*, are peculiar to it and unknown above, and *Agnostus* also becomes extinct, so that only nine genera continue upwards. The author, when he published his Preliminary Notice in 1846, knew of no species common to this and the higher division; but his attention having been drawn to the occurrence of such transitional forms in England by Sir R. Murchison, he made more diligent search, and now is acquainted with 20 well determined species which pass from the lower to the upper division of the Silurian formations. This is an important fact, as confirming the truth, now so apparent from the researches of English geologists, that the Silurian formations compose a great natural system, which cannot be divided without extreme violence. That the Bohemian quartzite strata belong to the lower division of the system is shewn by several characters: *First*, by the presence of the following genera of Trilobites, *Agnostus*, *Asaphus*, *Remopleurides*, *Dindymene*, *Illænus*, *Trinucleus*, *Ampyx*, all characteristic lower Silurian forms, though the last three also appear in the bottom of the upper division; *second*, by the great development of Orthis, as also happens in England, Russia, and North America; *third*, by the masses of Cystideæ analogous to those in the lower Silurians of the above countries, and also of Sweden and Ireland. There is thus a great harmony in the Silurian life of these distant regions, but with a few points of difference. Thus, in North America, and partly also in Scandinavia and Russia, Cephalopods greatly predominate in this division, especially forms of Orthoceratites with a large siphon, which are entirely unknown in Bohemia, in Britain, and in France. In many respects these Bohemian quartzites are most analogous to the Caradoc sandstones of England.* The analogy of this lower division of the Bohemian rocks to the Silurian formations of Scotland is also very remarkable, especially in their physical and petrographical characters.

These rocks compose the lower division of the Silurian rocks of Bohemia. The upper division forms only as it were a small oval island rising above their surface, and extending

---

* The following species are noted from both deposits: *Trinucleus ornatus*, Stern. *Illænus perovalis*, Murch. *Orthis redux*, Barr. *O. testudinaria*, Murch. *Orthis compressa*, Murch. *Bellerophon acutus*, Murch. The *Lingula attenuata*, Murch., from the Llandeilo Flags is also found in the Bohemian quartzites.

about five German (23 English) miles in length, by one mile (4·6 English) in breadth. It runs from Chodaun, about five English miles south-west of Béraun, to about three miles on the right bank of the Moldau, immediately south of Prague. The whole mass is calcareous with very few intercalated bands of schistose rocks, and is so continuous as to seem to render subdivision unnecessary. The comparison of the fossils, however, indicates four groups or stages, marked E, F, G, H, in ascending order.

The first stage E is from 150 to 300 metres thick. The basis is well marked by a great formation of trap rocks intercalated in black graptolite schists, with calcareous spheroids like those at the top of the lower division, and containing the same fossils. The traps seem to have been contemporaneous with the shales as they break through the inferior strata, but do not extend into the higher part of the series. The spheroids at first appear isolated among the black shales, but arranged more or less in beds; they then begin to alternate with thin calcareous layers with the same fossils, and at length approximate, and become changed into continuous beds of limestone. This limestone is of a black or dark-grey colour, sometimes compact, at other times crystalline, or in some beds almost entirely composed of remains of encrinites. It always exhales a disagreeable odour when broke, or is the Stinkstein of the Germans.

This group has, on the whole, the richest fauna both in genera and species of the entire series. In the Crustaceans, the Trilobites, indeed, fall off three in the number of genera from the preceding group, or 16 to 19, but increase by 25 in species, or 74 in place of 49. To eight old genera eight new ones are associated, completing the development of this tribe in the Silurian seas of Bohemia. No new forms are added in the higher rocks, the size of the species has evidently begun to decrease, and this family no longer predominates over the other races. On the other hand, the Cephalopods suddenly assume an immense development, attaining their maximum, both in the diversity of genera and species, and in the number of individuals. About 220 species are known, distributed in eight genera, as follows: *Nautilus*, 2 species; *Lituites*, 4 to 5 species; *Ascoceras*, 7 species; *Phragmoce-*

H 2

ras, 8 to 9 species; *Gomphoceras*, 14 to 16 species; *Trochoceras*, 14 to 16 species; *Cyrtoceras*, 65 to 70 species; and *Orthoceras*, 95 to 100 species. Some forms of the last two genera fill whole beds with their remains, but most of the other species, especially of the genera first mentioned, are rare. The occurrence of *Nautilus* in this early stage is, however, well established. The Pteropods of the genera *Conularia* and *Pugiunculus* have now undergone a marked decrease, and the Heteropods only furnish a few species of *Bellerophon*. The Gasteropods have their maximum in this stage, in 70 to 80 forms, of which 20 are referred to the genus *Capulus;* among the other types occur *Euomphalus, Natica, Murchisonia, Pleurotomaria, Turbo, Turritella,* &c. As a remarkable peculiarity may be noted one species of *Turbo?* and two of *Euomphalus*, furnished with their operculum. The Acephalæ also shew a richness of forms hitherto unexampled in Silurian countries; numbering about 80 species in the genera *Cardiola, Avicula, Mytilus, Cardium, Pleurorhyncus*, &c. &c. The Brachiopods also increase rapidly, 64 species being distributed in the following genera, thus: *Terebratula* 26, *Pentamerus* 2, *Spirifer* 12, *Orthis* 7, *Leptæna* 11, *Chonetes* 1, *Orbicula* 4, *Lingula* 1. A few species of Encrinites, but in incalculable numbers, represented the Echinoderms. The Zoophytes were from 25 to 30, including 10 or 12 Graptolites,—these problematical bodies disappearing in the lower part of this, as they only appeared in the upper part of the former stage. The genera of Polyparia observed in Bohemia, and even the species, are chiefly those known in England, as especially the *Calamopora, Porites, Cyathophyllum, Catenipora*, &c.

We already noticed the occurrence of Graptolites associated with several upper Silurian forms in some calcareous nodules, imbedded in the black slates of the last stage D. These beds are covered by strata more than 1200 metres thick, before the same species again appear in this upper stage. Either, therefore, the same specific forms have been twice created in this locality, or they have twice immigrated into this basin, at distant periods, from some other locality. The author adopts the latter view, and regards the earliest of these beings as a kind of colony sent out to explore an unknown region. This colony, with the matter in which it is imbedded, has, he thinks, come from the north-east, whereas the materials and the tribes characteristic of the earlier beds have come from the south-west. But if this view be admitted, it follows that, at the time the lower Silurian strata of Bohemia, with their characteristic forms, were deposited in that country, other strata, with an entirely distinct group of life, were forming in other parts of Europe. This result leads to questions of high interest to the geologist,—questions which are thus stated: 1*st*, How far can palæontological resemblance or identity demonstrate that formations geographically isolated from each other, are contemporaneous? 2*d*, How far does the dissimilarity between the faunas of isolated and distant basins correspond to the difference in the epoch of the deposits in which they are entombed?

The numerous races that peopled the seas of this period have almost entirely disappeared at its close. Several of the genera, indeed, continue, but only 21 of the species pass into the next higher group. Of the 220 Cephalopods, not one reoccurs in the following stage. And yet the two deposits are perfectly conformable, and their limit almost imperceptible, as if no great or sudden revolution had intervened.

The middle calcareous stage F differs, however, in mineral character, from that below. The fetid limestones have disappeared, and the colour of the rocks is less dark, frequently approaching to white. The calcareous spheroids have also vanished, though the limestone is often nodular, as shewn where it decomposes. It now likewise contains much silex, either disseminated through the rock or collected in irregular cherty masses.

Its fauna, too, varies much. The vital power of most classes seems to have decreased. The variety of generic types has notably declined, and still more the number of species. Of the Trilobites, there are ten genera, but with 72 species, the most numerous being in the two types *Proetus* (25 species) and *Bronteus* (19 species), both eminently characteristic of this stage, and next in *Phacops* (8 species.) Two species of Bronteus, *Br. palifer* and *Br. campanifer*, also equal in size the gigantic Asaphi and Paradoxides of the lower groups. The Cytherinidæ have here their maximum, both of numbers (8 species) and size, the latter unequalled either in the extinct or existing creation. The Cephalopods diminish in number, but the Goniatites (3 to 4 species) now appear in forms said to be very analogous to the Nautilus, and chiefly differing in the dorsal position of the siphon. The Gasteropods and Acephalæ have decreased greatly ; the Brachiopods, on the other hand, have now their maximum in 107 species (*Terebratula* 48, *Spirifer* 22, *Leptæna* 18, &c.) Encrinites seem also to have been abundant, and the Zoophytes, with some old, shew also many new forms, as *Retepora* or *Fenestella*, *Hemitrypa*, &c. Nineteen species pass from this into the next higher gronp.

The succeeding or upper limestone stage G is distinguished by thicker strata and a more argillaceous composition. The beds mostly consist of nodules of compact greyish limestone, with veins of calc-spar and masses of black chert in their interior. Thin pellicles of argillaceous matter envelope these nodules, and beds of highly laminated slate-clay separate the limestone strata, becoming more abundant and thicker towards the top of the stage. At the same time the limestone decreases in amount, and at length terminates in some spheroidal masses intercalated in the slates, in the same manner as it began below.

Animal life has still farther declined in this stage. Except the Trilobites, all the classes have almost disappeared, and even the remains of the few species that survived are singularly indistinct, the shells being, as it were, dissolved into the substance of the rock. The test of the Trilobites is much better preserved, small species being often found in the large chamber of an Orthoceratite, the shell of which has entirely vanished. The following are the genera of Trilobites, with the number of species,—*Harpes* 1, *Lichas* 1, *Calymene* 1, *Dalmania* 8, *Phacops* 6, *Proetus* 5, *Cyphaspis* 2, *Acidaspis* 4, *Cheirurus* 2, *Bronteus* 7. It is curious that these are the same genera which appear in the Devonian formations, and especially those of the Rhine and Hartz, where, however, the Calymene and Cheirurus are not certainly known. Only one species of animal, the *Phacops fœcundus*, is common to this and the next stage.

The upper stage, or the Culminating slates, H, appears only in a few isolated patches, sometimes, however, 100 metres thick, and was at first conjoined with the former group. It consists chiefly of grey or yellow slates of little consistence and mixed with a few beds of impure quartzite. In one place near Hostin, a thin layer of coal, 3 to 4 centimetres (1 to 1½ inch) thick, is seen among the slates. Here and there a few impressions of Fucoids or other plants, but very indistinct, occur, and are the only trace of vegetable life in the whole of this upper division. The fauna is extremely poor compared to the former groups, being comprised in a few species. The Trilobites have furnished one *Phacops* and one *Proetus;* the Cephalopods, two Orthoceratites and a Lituite; the Gasteropods some indistinct casts. The Brachiopods are represented by a *Leptæna* and a *Terebratula;* the Acephalæ by an *Avicula.* The Tentaculites are, however, pretty abundant in these localities. This paucity in the fauna may mark merely the continued decrease of life already noticed, or may depend on the want of calcareous matter in the rocks.

In comparing this higher division of the Bohemian Silurians with the formations of other countries, the author proceeds on the principles, that in these ancient seas, as in those of the existing world, some species were pretty generally distributed, though certain isolated basins might have a fauna in great part peculiar. A new species diffused from a point may also have appeared at different times in distant localities, and hence the occurrence of its remains in two isolated deposits is no proof of exact contemporaneity. Where many identical species occur, they, however, shew that the formations were not more widely separated than by the mean duration of a species, and were thus relatively contemporaneous. With these explanations, Bohemia shews many points of analogy with other Silurian countries. These relations do not depend on geographical proximity, for the deposits of Franconia have less analogy to those of Bohemia than those of England and Sweden, and scarcely so much as those of North America. Again, these relations vary from age to age. Thus the protozoic group C, with the same or corresponding generic types, has yet no common species with

England or Sweden; in the quartzites D, the analogy is notably increased, and many forms are identical; whilst in the superior division these identities are very numerous and extend over all the classes. It thus appears that in the period of time represented by the Silurian formations of Bohemia, certain modifications on the surface of the globe have successively increased the facility of communication between these three countries. In his special comparison of the Bohemian fauna with that of other countries, the author gives the chief place to England and Sweden, both from their greater analogy, and because these countries are now rendered classical in geology by the works of Sir R. Murchison and other scientific observers.

The author (p. 88-92) gives a table of the species common to these three countries, but we can only note some of the general results. First, the Crustaceans are few in numbers, perhaps a result of their feeble means of transport, and the conditions which seems to tie down the species to narrow limits. On the other hand the Cephalopods, great swimmers, should be widely spread. This is seen in Bohemia, but the bad preservation of the English and Swedish specimens does not admit of exact comparison. The forms of Phragmoceras, however, in all the regions confined to a short period, impress a very decided character on the epoch. Again, the great superiority in the number of identical species among the Brachiopods depends chiefly on the easy diffusion of such light shells, by means of currents, and partly on the greater attention given to this class of late. The Polyparia seem also to present many common species, though the list is not yet complete. Their great vertical extent, however, renders this fact of less importance.

On considering the table attentively, it appears that by far the larger portion of the common species occur in Bohemia in the lower limestone stage E, in England in the Wenlock beds, and in Sweden in the Gothland formations. These, therefore, are contemporaneous deposits, though as some of the Bohemian forms ascend into the Lower Ludlow, this limit must not be taken too strictly. On the other hand the upper three stages, F, G, H, have few species in common with England, and hence the seas then were probably more isolated. In Sweden no formations higher than those of Gothland occur. It is curious that whilst the Lower Silurians of France shew no analogy with the lower limestone of Bohemia, it is in that country that the greatest number of species identical with the middle limestone stage F have been found. Viré in the Sarthe Dept has furnished already eight species.

In North America no identical species have been noted from the lower rocks, but from the upper division 21 species are found in M. de Verneuil's lists. Almost all of these are from the Clinton and Niagara groups ; and further examination will probably shew more numerous identities.

The countries nearest to Bohemia on the continent are still so little known as to permit only of a very imperfect comparison. In the Fichtelgebirge he seems to regard the Orthoceratite and Clymenia limestones as approximating to his stage E ; the first deposit being probably the lower and Silurian, the second higher and Devonian. The Thuringer Wald and Saxony contain ancient rocks, but the materials of comparison are still wanting.

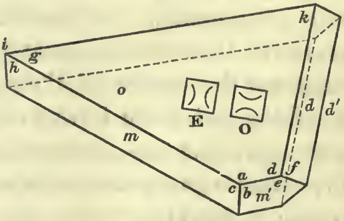
This notice of a few of the more general results which M. Barrande has deduced from his study of the ancient rocks of Bohemia will, we trust, induce some of our readers to look into the work itself, and to encourage the author—an exile in a distant land—to proceed in his researches, by promoting the sale of his book. The account of the classification, development, and geological distribution of the Trilobites is singularly interesting, the abundance of materials which he has collected, with great labour and expense, having enabled him to add many new facts to the history of this long extinct race of animals. As the Silurian formations formed the centre of their development in time, so Bohemia might be supposed the centre of their geographical distribution, did not facts which we have recently seen prove that parts of our own land are perhaps no less rich in these curious Crustaceans, were they only sought with equal diligence. In regard to the other tribes, his work will, we expect, furnish materials scarcely less valuable to the student of the ancient rocks.

*On the Optical Properties of a Compound of Iodine and Codeine.** By WILLIAM HAIDINGER, Esq., Councillor of Mines in Austria, and Professor of Mineralogy, F.R.S.E., Member of the Wernerian Nat. Hist. Soc. of the Royal Academies of Berlin, Munich, &c., &c. Communicated by the Author.

The crystals which I now submit to the notice of the physico-mathematical section of the Academy belong to the class of those which reflect the incident ray with coloured polarization, while the transmitted ray presents a colour different from, and complementary to, that reflected from the surface. They form an individual example of a series of substances exhibiting all the modifications of the colour spectrum in their transmitted, reflected, internal, and surface colours, with which I have been engaged for some time past, and which I hope soon to be able to submit to the Academy in a connected form. These crystals, however, appear to me to deserve an earlier notice, as they are the result of an entirely new, and, as yet, incomplete investigation of Dr Anderson of Edinburgh, by whom they have been transmitted to me through the hands of my colleague, Professor Schrötter.

The crystals are tabular, apparently equilateral triangular plates, and, at first sight, I was inclined to seek for a rhombohedral form in the arrangement of the narrow faces distributed on the edges. A more minute examination, however, proved them to belong to the Anorthoid system, and their form is shewn in the accompanying figure. If we assume the large face O as the terminal plane or base of the crystallographic series, *m* and *m′* may be considered as the left and right faces of a rhomboidal prism, the limit of the series of the anorthoid forms, that is as $l \infty A/2$, and $r \infty A/2$. Of the two faces $l \infty A/2$ only the anterior + face occurs, the posterior −

Fig. 1.



* From the "Sitzungsberichte" of the Vienna Academy of Sciences.

face is entirely absent. The faces d and d' may be considered as the macrodiagonal hemidomata + r H/2, and − l H/2, of which, owing to the unsymmetrical development of the crystals, the opposite faces + l H/2 and − r H/2, are wanting. In the position of the acute edge next the angle g, the plates are frequently united together so that that edge disappears, and the crystals then diverge in the form of a fan. The size of the crystals I examined was about three lines on the longest edge, and their thickness about a sixth of a line.

I am indebted to Mr Franz Foetterle, Inspector of Mines, for the following measurements, taken with the reflecting goniometer:—

Inclination of	o on m	=	131° 5′
,,	o ,, m'	=	116° 15′
,,	d ,, d'	=	77° 42′
,,	o ,, d	=	141° 9′
,,	o' ,, d'	=	141° 9′
,,	m ,, $m_{,}$	=	147° 0′
,,	,, m	=	128° 0′

From these he has also calculated the following plane angles:—

a	=	143° 58′
b	=	125° 57′
c	=	74° 39′
d	=	118° 51′
e	=	135° 35′
f	=	85° 58′
g	=	36° 2′
h	=	105° 26′
i	=	125° 57′
k	=	61° 9′

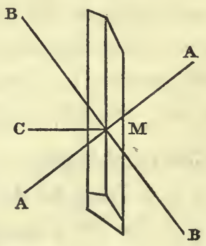
The combination edge $o\,d$ forms, with the combination edge $o\,m'$ lying to its right, an angle k of 69° 9′, and with that to the left $o\,m$ an angle of 82° 49′, and the base consequently assumes a rhomboidal form when lines are drawn through the obtuse angles parallel to these edges. The obtuse angles of the rhomboid are = 143° 58′, the acute = 36° 2′; the diagonals intersect each other under angles of 104° 24′ and 75° 36′; they divide the obtuse angle into two angles of 83° 10′ and 61° 3′, and the acute angle into two of 21° 35′ and 14° 27′.

The inclination of the faces *d* and *d′* to the adjacent terminal planes appears perfectly equal.

The whole of the measurements succeeded tolerably well, as the faces though small are smooth and brilliant, with the exception of that marked m (+ $l\infty$ A/2), which only occurred irregular.

The triangular plates have a brown colour, which passes into a rich deep orange when they are reduced to powder. When thin they are perfectly transparent; they have a fine adamantine lustre. In order to examine their pleochroism by means of the dichroscopic lens, they are most advantageously attached to a piece of wax by the acute angle at *g*, and held so that the edge *d d′* may be horizontal. The ordinary image O is then seen above, and the extraordinary E below, as represented in fig. 1. By perpendicular incidence of the light the ordinary ray appears much lighter than the extraordinary, and the tint varies with the thickness of the plate, the former passing from a pale yellowish-brown, through dark honey-yellow into blood-red, while the latter commences with blood-red and soon becomes opaque, that is, gives a black image. When the crystal is made to rotate towards right or left without deranging the horizontal position of the edge *d d′*, we observe a gradual increase or decrease of transparency; and it appears most transparent in the ordinary image, when examined in the direction of AA, fig. 2, that is, nearly perpendicular to the edge between *m* and *m′*, or perpendicular to the axis of the prism. It is least transparent in the direction of the axis BB. From this it follows that only one of the axes of elasticity of double refraction lies in the plane of the triangular plate, and that perpendicular or nearly so to the edge *d d′*, while the other two, which are perpendicular to one another, form with the section of the base, angles such that CMA is about 30°, and CMB about 60°. The colour polarized in the direction AA perpendicular to BB is the brightest, that polarized in the direction BB of interme-

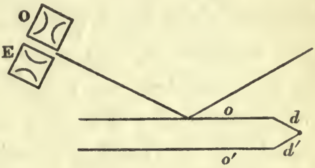
Fig. 2.



diate brightness, and that perpendicular to the intersection of the two planes AA and BB the darkest. All have, however, the same fundamental dark orange-colour, and are distinguished only by differences of intensity.

The adamantine surface-lustre, when examined by reflection with the dichroscopic lens, is decomposed in such a manner, that a portion of the reflected light is definitely polarized (*fest polarisirt*) in the direction of the edge *d d'*, or as E in fig. 1. In the position represented in fig. 3, all the ordinary polarized light passes into the upper image, the extraordinary polarized into the lower image, and the contrast is then most complete. In the position perpendicular to this, all the definitely polarized light, along with the white surface lustre, passes into the upper image. In the position represented in fig. 3, the colour is not equally blue at all angles of incidence. When the angle becomes considerable, the colour passes into violet, and when it becomes very high, an imperfect bronze yellow makes its appearance in the lower, contrasted with a bright white in the upper, image.

Fig. 3.



The crystals now described afford a new confirmation of the law already laid down by me in the second part of the "Sitzungsberichte," viz., that the determinate surface-lustre, or the definitely polarized surface colour, corresponds in the direction of its polarization with that of the more absorbed ray of doubly refracting crystals.*

According to Dr Anderson, the chemical constitution of this substance is not yet fully determined, and in default of a systematic name, I propose to designate these crystals, so in-

* As there is some difficulty in translating this law so as exactly to express Professor Haidinger's meaning, we give it here in the original, in which it stands thus:—"Der orientirte Flächenschiller, oder die fest polarisirte Oberflächenfarbe in der Polarisationsrichtung mit der Polarisationsrichtung der mehr absorbirten Strahles doppelt brechender Krystalle übereinstimmt."

teresting in an optical point of view, by the name of Andersonite. Had this been a substance occurring in nature as a mineral, this would have been a proceeding of which we have already numerous examples, but the introduction of the practice in a branch of science, which we are in the habit of looking upon as altogether separated from mineralogy, may be considered as an innovation. In our knowledge of inorganic substances, however, we have still so much to ascertain, that even here the necessity for independent specific names becomes daily more apparent. Amidst the large number of new substances, any general attempt in this direction would be as thankless as it would be troublesome, and probably unsuccessful, for it is only at some future period that the impossibility of avoiding it will become fully apparent. In the meantime, we may advantageously provide for individual cases. I have long wished, ever indeed since I commenced the study of these substances, to associate the names of the chemists of the day with the wonderful phenomena of the crystals with metallic surface-colour, to call the yellow platino-cyanide of barium, Redtenbacherite; the carmine-red, platino-cyanide of magnesium, with the green surface, Quadratite, suggestive also of its pyramidal form; while the prismatic platino-cyanide of magnesium, with aurora-red colour and blue surface, might be called Aurorite. Knop's platino-cyanide of potassium would be called Knopite; Schunk's chrysammate of potash, Schunkite; Gregory's oxalate of chromium and potash, Gregorin (the name of Gregorite for the Cornish titaniferous iron, though long in disuse, can scarcely be considered as altogether free); and here Andersonite would distinguish the still incompletely-examined compound of iodine and codeine. These investigators have unquestionably contributed much more to the progress of science than by the individual facts with which I wish to associate their names; all that I am desirous of doing, is to establish a principle for the fulfilment of a want, which is every day becoming more apparent.

A New Theory of the Central Heat of the Earth, and of the cause of Volcanic Phenomena. By Mr STEVENSON MACADAM, Teacher of Chemistry, Philosophical Institution, Edinburgh. Communicated by the Author.*

The projection of matter in a fused state from the craters of volcanoes, and the emission of hot water from certain springs, led philosophers, in early times, to suppose that a high temperature prevailed in the lower regions of the earth. This has been latterly distinguished as the "central heat" of the globe. In later years this opinion has been strengthened by the rapid increase in temperature observed to be exhibited by the constituents of the earth's crust, as we descend from the superficial to the lower strata. This augmentation in temperature has been ascertained, by thermometrical observations made in deep mines, wells, &c., to be such that, according to Cordier, it is not overstated at 1° F. for every forty-five feet of depth.

The observations on this increase, although they extend to a comparatively insignificant depth in the terrestrial crust, furnish data by which, provided we assume that the increase in temperature is at the same rate, at great as at small depths, we may calculate the temperature at any distance from the surface. Certain geologists, accordingly, calculating in this way, have come to the conclusion that the intensity of heat of the central nucleus will not be less than 450,000° F. Others, again, placing a limit to this rise of temperature at a certain depth, are of opinion that a cessation of this increase occurs there, and that all matter placed betwixt that point and the centre of the globe is possessed of an uniform temperature.

The advocates of the theory which supposes that the heat continues to increase to the centre of the globe, assume the possibility of a crust of solid matter, at a comparatively low temperature, enclosing a mass of liquid at a very high temperature; whilst the thickness of this crust bears the same

* This Memoir, in a more extended form, was laid before the meeting of the British Association at Edinburgh, 1850.

proportion to the mass of the earth, which a sheet of paper does to that of a medium-sized geographical globe.

This theory is evidently quite inconsistent with our present knowledge of the laws of liquefaction. For when a piece of solid matter is added to a portion of the same substance in a state of fusion, and at a temperature above its melting point, the solid, if not too great in bulk, soon becomes liquefied. Those, therefore, who contend that the central liquid is at a greatly higher temperature than the crust which encloses and is in physical contact with it, assert a condition of matters which all experience contradicts, and which is as inconceivable as that a crust of ice should form or remain on the surface of hot water, or a solid film of iron on the white-hot liquid metal.

The adherents of the theory which places a limit to the increase in temperature, contend that the crust of the globe merely lies on the liquid mass, as we find a portion of ice does on ice-cold water.

This theory presupposes that the solid materials composing the crust are, as in the case of ice and water, specifically lighter than the liquid portion, and, on the same principle that a cake of solid water can rest or float on a lake of liquid water, so in regard to the earth, it is possible that the crust can float upon the liquid mass. So far, accordingly, as central heat is concerned, this theory might be recognised. If, however, we look at what it can tell us regarding the density of the globe, we shall there find it at fault.

The density of the earth, as experimentally determined, is about 5½ times that of water. On comparing this with the mean density of the superficial parts of the crust, we find that their density is only about one-half that of the entire earth. But if we take into consideration the augmentation in weight which substances at some depth must undergo, owing to the increased pressure towards the centre, we are justified in asserting that the globe cannot be, as is supposed by the advocates of the theory in question, a homogeneous liquid sphere, in physical contact with the crust which encloses it, for, if it were so, it would far exceed the determined density. It is requisite, on this view, to assume that some powerful agent of expansion, such as heat, is at work counteracting the enormous pressure of the superincumbent mass, or that a space or spaces should be satisfactorily accounted for. The theory under discussion admits of neither of these alternatives. The liquid cannot be raised in temperature without the crust becoming fused, and as for a space filled with vapour, this theory does not admit of one.

From the foregoing remarks it may be deduced, that while it has been satisfactorily demonstrated, that a rapid increase in temperature towards the centre of the globe does take place, which would lead us to believe that the greater portion of this globe is in a state of fluidity, nevertheless the two theories which have been discussed, though they take cognizance of, and account for this increase in temperature, yet cannot be accepted, as they severally assume what appears clearly impossible.

My object in the following remarks is to shew that the apparently incompatible phenomena of a solid crust at a low temperature, enveloping a liquid at a higher one, may be realized.

The view which I am about to propose is founded upon the assumption by matter when raised in temperature of the peculiar state distinguished as the spheroidal; and it will conduce to perspicuity, if I commence with a brief reference to the recent researches on this subject.

For the greater part of the knowledge we possess, regarding the phenomena presented by bodies when they assume the spheroidal state, we are indebted to the laborious investigations of M. Boutigny. The chief points of interest in relation to the present inquiry observed by him are the following,—water being selected as the example of the body exhibiting, when heated, the spheroidal state.

Few phenomena are more familiar than that of water, when placed at ordinary temperatures in a metallic or other vessel, wetting its surface and spreading over it, and we are equally familiar with the effect of elevation of temperature in such circumstances, in dissipating the water in vapour. If, however, instead of raising the temperature of the vessel, after the addition of the water, the vessel be first raised to a temperature of not less than 340° Fahrenheit, and some water be then projected into it, the liquid does not, as might be expected, wet or spread over the vessel, and then pass into a state of violent ebullition, but, as it were, rolls itself up and suddenly forms a globule or sphere, like a dew-drop, and moves about from side to side of the vessel which contains it. *The water in these circumstances is said to have assumed the spheroidal state.*

The sphere of water is not in contact with the heated vessel, but executes its movements at a sensible distance from its surface. The temperature of the water, moreover, is not at its boiling point, and therefore evaporation proceeds slowly, so that a measured quantity of water which would, by ordinary ebullition at 212° be dissipated in vapour in one minute, if in the spheroidal state in a vessel at 340° Fahrenheit would take 50 minutes to evaporate. Such are the chief phenomena attending, or characteristic of, this singular condition of matter.

Boutigny made similar observations upon many other liquids, as well as on various solids, and arrived in consequence at the following conclusions:—

1*st.* That all bodies can pass into the spheroidal state.

2*d.* That the temperature of bodies in the spheroidal state, whatever be the temperature of the vessel which contains them, is invariably inferior to their point of ebullition.

3*d.* That there is no contact between bodies in the spheroidal state and the surfaces of the heated vessels on which they are placed.

4*th.* That bodies in the spheroidal state exhibit absolute reflection in regard to heat.

The foregoing conclusions are the results of experiments performed in heated shallow vessels, into which the substances to be acted upon were projected. This, however, is not the only way in which the experiment can be performed. If a rod of platinum be raised to a *white heat*, and then plunged into water, for a time no hissing sound is heard, such as attends the quenching of *red hot* platinum in water, nor is there any burst of steam such as accompanies sudden ebullition. This is in consequence of the water assuming the spheroidal state, and being repelled by the white hot metal. After a certain interval, however, the temperature of the platinum falls sufficiently to allow the liquid to wet it, and then, for the first time, the hissing sounds and burst of steam, characteristic of sudden ebullition, are observed.

In such an experiment as the one just recorded, the condition of the water next the hot metal is the same as that of drops of the same liquid thrown upon a heated surface, and the water is said to assume the spheroidal state in the former, as well as in the latter case, although in the first it does not exhibit itself in a single sphere, or in several perfect spheres. In what follows I shall have occasion to refer to each of these manifestations of spheroidicity.

Thus much then premised, I shall now proceed to the more immediate object of this paper, viz.,—the application of the results of the observations on bodies in the spheroidal state to the physical constitution of the globe.

I assume that our globe at the present time consists essentially of three distinct portions :—

1*st.* A central nucleus in a state of igneous fusion.

2*d.* A crust at a comparatively low temperature, the inner side of which is in the spheroidal state.

3*d.* A space between the crust and the central nucleus, possibly filled with vaporised mineral matter.

The arrangement of these several portions and their connection one with another, may be better understood by reference to the constitution of an *egg*, which bears a strong analogy to it in point of arrangement, though differing in shape. The yolk of the egg represents the mass of matter in a state of igneous fusion; the white of the egg, the space between the heated mass and the crust; and the shell of the egg, the crust of the globe.

When referring to the experiment with the platinum rod, I stated that when it was heated to the required temperature and plunged into water, the liquid did not touch the rod, but was seemingly repelled by it, and that, therefore, a space intervened between the rod and the water. In the proposed theory, I make no difference in point of assumed arrangement, but merely the substitution of one kind of matter for another. In the experiment there are the heated rod, the space, and the water in the spheroidal state; in the globe there are the hot nucleus, the space, and the crust, the inner side of which is in the spheroidal state.

The crust of the globe, as thus circumstanced, will be influenced by two great forces, viz.:—gravitation and spheroidal repulsion; the former tending to draw the crust towards the central nucleus, the latter repelling it from it. The crust will, therefore, have assumed the position where the equilibrium of the two forces is established,

I do not enter at present into the consideration of the relative dimensions of the several portions of the globe, but, considering the rapid increase in temperature from the surface of the crust towards the interior, it is not likely that the crust will exceed twenty-five miles in thickness. The innermost layers of the crust will possess a high temperature which will gradually decrease towards the surface of the earth.

There is one important feature which all bodies in the spheroidal state present, and which I wish to bring prominently forward. I refer to the remarkable property of total reflection of the heat incident upon them. The effect of this property must be to make the inner surface of the crust of the globe (which, it will be remembered, is in the spheroidal state) equivalent in every direction to an immense concave mirror, whose temperature will be very slightly affected by the heat which falls upon it. Such a condition of matters is manifestly compatible with the presence of a much higher temperature at the central nucleus than at the inner surface of the crust, and necessitates a much slower cooling of that crust, and consequently of the nucleus which it robs of heat, than would be the case if the power to reflect heat were not characteristic of the spheroidal condition of matter. And seeing that at a certain, though varying, distance from the surface of the earth, there is an invariable temperature in every latitude, the spheroidal condition of the inner surface of the crust must be considered permanent, so long as our present cosmical arrangements continue unchanged; for as the crust has long ceased to vary in temperature, except within a small distance from the surface, there is no force at work which can deprive the inner surface of the crust or the central nucleus of heat, and, therefore, none which can diminish their spheroidicity. They must be regarded as two surfaces constantly exchanging the same amount of heat, and prevented, in consequence, like two equally warm radiating bodies on Pictet's theory, from undergoing any change in temperature.

Such is the theory by which I seek to reconcile the coexistence in our globe of a central heat and fluidity, and a cold solid crust.

The harmony of this theory with the known density of the globe, may now be advantageously alluded to.

In a former part of this paper I stated that, in order to reconcile the mean density of the globe with that of its constituents, it was requisite that some powerful expansive agent, or a space filled with vapour, should be accounted for, so as to lower the mean density of the earth.

In the spheroidal theory of the earth there are two different ways by which the density may be lowered, 1st, by the dilatation of the central fluid consequent upon its high temperature; and 2d, and more particularly, by the space which intervenes between the central nucleus and the external crust. The former, viz., the expansion of the materials in the liquid state, would go but a small way in lowering the globe's density; when, however, the space which this theory necessitates is taken into consideration, there is ample room afforded by which the density of the globe, as a whole, might be more or less lowered.

The other planets are probably in the same condition as the earth, in so far that each has a liquid central nucleus at a distance from the enclosing crust, and in consequence, a lower density than it would have if solid to the centre; but, in our ignorance of the chemical composition of the central portions of our own globe, and of the entire mass of the other heavenly bodies, as well as of the temperature which characterises their inner portions, we are without the data necessary for calculating what their specific gravity should be to accord with the requirements of the Spheroidal Theory.

I shall now proceed to offer a few remarks regarding the principal theories which have been promulgated, in order to give an explanation of the causes of volcanic phenomena.

The theories referred to are respectively termed the mechanical and the chemical; the former assuming that volcanic eruptions are but the exudation of the interior fused mass through the crust,—the latter believing that they can be accounted for by the chemical action and reaction of certain elementary substances.

The adherents of the mechanical theory have put forward many suggestions, in order to account for the propelling power by which the molten mass could be ejected through the crust. Humboldt considers that "all volcanic phenomena are probably the result of a permanent or transient connection between the interior and the exterior of our planet. Elastic vapours press the fused oxidising substances upwards through deep fissures. Volcanoes therefore are intermittent earthsprings."*

The mechanical theory has thus, it would seem, for its foundation, the supposition of a central nucleus in a state of liquidity in physical contact with the crust. As this theory has been assumed to be untenable, for the reasons alluded to at the beginning of this paper, any explanations of volcanic phenomena based upon it must inevitably fall to the ground.

Many objections have also been made to the sufficiency of the chemical theory, to account for the phenomena in question. These objections have received considerable weight, from the circumstance that the talented chemist who advanced the theory afterwards saw reason to relinquish it. If, however, his principal reasons for doing so be carefully considered, and recent scientific discoveries be brought to bear upon them, there seems, I think, every probability that the chemical theory will be found to be a more satisfactory one than its promoter latterly considered it.

The following sentence, quoted from Sir Humphry Davy's works, will explain his reasons for losing faith in the theory referred to:—"There are, however," says Davy, "distinct

---

* Views of Nature. Humboldt, p. 373.

facts in favour of the idea that the interior of the globe has a higher temperature than the surface ; the heat increasing in mines the deeper we penetrate, and the number of warm sources which rise from great depths in almost all countries, are certainly favourable to the idea."*

From the above quotation, which embraces the substance of all objections as yet brought forward, it would appear that they are founded upon the fact, deduced from observation, that there is an internal heat for which at the present time no satisfactory explanation can be given by the advocates of the chemical theory. That chemistry has as yet failed to give the explanation referred to, is beyond doubt, but that this should render null and void the probability of the chemical theory accounting for volcanic phenomena, is carrying the inference rather too far. No substantial objection has, so far as I am aware, been brought forward against the probability of *volcanic phenomena* being caused by chemical action and reaction ; whereas many objections have been directed to shew that the production of the *central heat* by chemical means was untenable, and then the inference was drawn that both effects were the result of one cause, and that what failed to account for the one could not account for the other.

Internal heat and volcanic phenomena I ascribe to two different causes ; and while I believe that chemical action is quite competent to account for volcanic phenomena, at the same time I conceive that the internal heat is not directly due to this force, but is the inevitable consequence of the matter composing the crust being at such a short distance from the central nucleus. In the spheroidal theory of the earth such an internal heat is fully accounted for, in the proximity of the crust to the central nucleus. But while the heat of the inner portion of the crust is attributable to the central nucleus, there is no physical contact, and therefore little or no possibility of fused matter being ejected from the central sphere through the crust.

---

* Collected works of Sir H. Davy, by Dr John Davy.—Consolations in Travel, Dialogue 3, p. 295.

Before proceeding further, I shall here make reference to an experiment, as the results which it gives appear to illustrate what occurs in volcanic phenomena.

If a shallow vessel of copper be raised to a temperature of about 400° Fahr., and some drops of nitric acid be projected into it, the nitric acid (like water placed in the same circumstances) instantly assumes the spheroidal condition, and rolls about the capsule without any visible chemical action upon it. During the time that the nitric acid is in the spheroidal state, there seems to be a complete cessation of that violent chemical action which characterises the meeting of nitric acid and copper at ordinary temperatures. The duration of the spheroid of nitric acid depends on two circumstances: first, that the heated vessel be directly kept at the required temperature by lamp or otherwise; and, secondly, that there be a moderate quantity of liquid. For, should the copper vessel be reduced in temperature, either directly, by withdrawing the source of heat, or indirectly, by adding more liquid than the heated vessel can possibly keep in the spheroidal state, the nitric acid will wet the surface of the copper, violent chemical action will be the consequence, and volumes of nitric oxide gas be given off. From the above experiment it would appear that—

1*st*. There is no chemical action between bodies in the spheroidal state and the hot surfaces over which they are placed.

2*d*. That chemical action instantly takes place when the vessel is directly reduced in temperature, or an overplus of liquid added; in other words, when physical contact is determined between the two bodies.

In applying the preceding experimental results to volcanic phenomena, I assume, that there exist contiguous to the volcanoes of our globe (either formed or in the act of formation) basin-shaped cavities, more or less deeply seated, the under part of which is composed of metallic bodies at a high temperature. Water, either from lakes, &c., at the surface, or from subterranean reservoirs, finds access to one of these cavities. The first portion which descends instantly assumes the spheroidal condition,—more water enters, and still it is spheroidized,—the stream continues, till, in course of time, an immense volume of water is there rolling and tossing about, but not yet touching the metallic surface; ultimately, however, the balance is overturned, the liquid touches the metallic basin. An immense volume of water is thereby quickly converted into steam; while at the same moment chemical action on a large scale speedily ensues between the liquid and the metallic bodies,—the latter action giving rise to heat quite sufficient to fuse large portions of mineral matter. The almost instantaneous generation of large volumes of vapours and gases, and these promptly augmented in bulk for some time, would soon produce a force quite able to raise large tracts of land, and, when a vent was made or obtained, would eject the fused mass, as seen from the craters of some modern volcanoes during the term of an eruption.

By the above theory we are put in possession of a method by which a very powerful and long-continued force can be generated, and brought to bear upon volcanic phenomena. When the vapours began to be produced, the water-course would be dammed up, so that during the term of an eruption no additional quantity of water could be projected into the basin-shaped cavity from the water-stream: when, however, the whole of the water in the cavity had been converted into the gaseous condition, and the force had gradually spent itself, then the water would once more begin to percolate into the cavity. Of course, some time would elapse after the last drop of water was rendered gaseous before the pressure of the gases would allow the water to issue from its course; during this time the basin-shaped cavity would be regaining, from the surrounding hot matter, any heat which it had lost during the eruption. Matters would thus again resume their former aspect; the water would once again become spheroidized in the subterranean cavity, there rolling and tossing about, but waiting the appointed time, when the balance will be overturned, and the striking phenomena characteristic of a volcanic eruption will again shew themselves.

Observations made on a Voyage from Leith to Grenada. By PATRICK GILLESPIE, *Commander of the Barque, Marli of Leith.*

EXPLANATION OF THE TABLE.—These observations are divided into 16 columns. The 1st contains the date; the 2d column shews whether the observations were made at noon or midnight. The 3d, 4th, and 5th columns contain the readings of the Barometer, Adie's Sympiesometer, and the Aneroid Barometer. The index errors of the two latter instruments are indicated in the first observation, the Barometer being the standard. The 6th column, marked T. A. (temperature of the atmosphere), contains the observations made from a thermometer hung in the companion, shrouded from the influence of the sun's rays, and indicating, as accurately as on board ship can be obtained, the temperature of the air. The column headed T. W. (temperature of water), exhibits the indication of a thermometer by J. Bryson, Edinburgh, the bulb of which is coated with bee's wax, so as to render less rapid any increase of temperature in removing it from the water to the warmer air. In lower latitudes this precaution is less necessary, as a glance at the observations will shew the nearer approximation of the temperatures of the water and air, in some instances the former being higher than the latter. Column No. 8, headed D. W., shews the average direction of the wind from morning till noon. Column No. 9 shews the force of the wind; 60, the maximum, is reckoned a hurricane; 50 a gale, and so decreasing to zero a calm. Column 10 indicates the amount of clouds from horizon to horizon; 10 being total obscuration. Zero cloudless. The 11th column shews whether it has been rain or fair at the hour of observation. Column 12 denotes the latitude, as reckoned either by account or observation shewn in column 13. Column 14 shews the longitude at the time of observation, either by account or otherwise, as shewn in column 15. The 16th column is devoted to remarks which may be interesting to those acquainted with meteorological or maritime pursuits, as it exhibits the changes of the sails with the varying phases of the weather, as shewn by the instruments.

1	2	3 Barom. Inches.	4 Sympie. Inches.	5 Aneroid Inches.	6 T. A.	7 T. W.	8 D. W.	9 F. W.	10 Clouds.	11 Rain.	12 Latitude.	13	14 Long.	15	16 Remarks.
1849.					°	°					° ′		° ′		
Dec. 21	N.	30·50	30·65	30·51	52	49·0	S. W.	30	10	Fair.	59·00	N. acct.	7·42	W. acct.	Heavy sea; ship under double-reefed topsails and courses.
22	...	30·45	30·63	30·53	52	49·0	...	50	10	Light rain.	59·35	...	11·27	...	Ship under close reefed topsails and reefed foresail.
23	...	30·60	30·82	30·63	54	50·0	WNW.	10	9	Fair.	59·35	...	12·47	...	All sail set; heavy S.W. sea.
24	...	30·75	30·82	30·71	53	50·0	variable.	4	10	...	58·09	...	12·23	...	Fine frosty weather; all sail set.
25	...	30·78	30·88	30·65	52	52·0	NW.	20	8	...	56·19	N. obs.	12·35	...	Fine weather.
26	...	30·40	30·52	30·39	52	52·0	...	30	5	Showery.	53·01	N. obs.	14·52	...	Ship under single-reefed topsails.
27	. .	30·20	30·31	30·18	55	55·0	N.	25	4	...	49·46	...	17·25	...	All sail set; steady breeze.
28	...	30·28	30·33	30·23	56	54·5	NE.	20	5	Fair.	46·36	...	17·50	W. chro.	Fine weather; all sail set.
29	...	30·25	30·30	30·17	56	57·5	ESE.	15	3	...	43·50	...	18·56	...	Fine weather.
30	...	30·20	30·27	30·13	60	62·0	S by E.	20	9	...	41·12	...	20·54	...	Single-reefed topsails; heavy swell from N.
31	...	30·31	30·33	30·26	62	64·0	SSE.	15	6	...	39·30	...	21·55	...	

1850. Jan. 1	N.	30·46	30·48	30·28	65	64·0	E.	15	4	Fair.	36·39	N. obs.	22·30	W. chro.	Fine weather; all sail set; 19 days from Leith and 12 from Stromness.
2	...	30·32	30·29	30·10	65	66·0	...	17	9	...	33·41	...	22·51	...	
3	...	30·15	30·26	30·09	67	68·0	ENE.	15	2	...	30·18	...	24·33	...	
4	...	30·30	30·33	30·22	71	70·0	E.	25	6	Showery.	28·15	N. acct.	27·18	...	Heavy squalls, with rain.
5	...	30·22	30·29	30·12	71	72·0	ESE.	30	7	...	25·48	N. obs.	30·34	...	
6	...	30·10	30·12	30·00	74	74·0	...	35	5	...	23·36	...	33·37	...	Heavy squalls, with heavy sea.
7	...	30·00	30·06	29·97	75	75·0	SE.	35	10	...	21·25	...	36·43	...	Ship under ingle-reefed topsails.
8	...	29·96	30·03	29·89	75	76·0	...	25	9	...	19·25	N. acct.	40·01	...	
9	...	29·98	29·95	29·86	83	78·0	...	15	3	Fair.	18·08	N. obs.	42·28	...	Fine weather; all studding sails set.
10	...	29·95	29·88	29·81	82	79·0	Variable.	Light	7	Showery.	16·50	...	45·09	...	... ...
11	...	29·90	29·85	29·78	80	79·0	E. by N.	8	8	Fair.	15·51	...	47·01	...	... ...
...	M	29·90	29·80	29·80	76	77·0	...	5	—	...	15·33	N. obs*	48·31	...	... ...
12	N	29·90	29·86	29·78	80	79·0	ENE.	7	1	...	15·22	...	49·04	...	... ...
...	M	29·80	29·90	29·80	76	77·0	...	9	1	...	14·49	N. acct.	49·20	W. acct.	... ...
13	N	29·90	29·82	29·79	80	79·0	E.	12	1	...	14·28	...	50·00	W. chro.	... ...
...	M	29·90	29·81	29·85	77	78·0	ESE.	20	8	Rain.	14·17	...	51·01	W. acct.	... ...
14	N	29·90	29·85	29·82	80	79·0	...	20	2	Fair.	14·23	N. obs.	52·00	W. chro.	... ...
...	M	30·00	29·82	29·85	77	78·0	...	15	5	...	14·04	...	53·00	W. acct.	... ...
15	N	29·90	29·80	29·78	82	79·0	...	12	2	...	14·00	...	54·14	W. chro.	... ...
...	M	29·90	29·90	29·85	78	79·0	...	0	9	Rain.	13·45	N. acct.	54·49	W. acct.	... ...
16	N	29·89	29·87	29·82	82	80·0	NE.	6	4	Fair.	13·44	N. obs.	55·12	W. chro.	... ...
...	M	29·90	29·92	29·85	76	78·0	...	15	6	...	13·23	N. acct.	56·37	W. acct.	... ...
17	N	29·95	29·91	29·84	81	79·0	...	12	2	...	13·16	N. obs.	57·36	W. chro.	.. ..
...	M	29·90	29·94	29·90	77	78·0	...	15	4	...	13·02	...	59·11	...	
18	N	29·95	29·93	29·88	81	78·0	E.	15	2	...	13·00		60.00	W. acct.	Passed Barbadoes this morning; chronometer No. 34, right to a mile; No. 28 wrong 14½ miles to the west.
21	...	29·98	29·90	29·85	81	79·0	...	15	5	Showery.	At Grenada.				

On the Chemical Constitution of the Rocks of the Coal Formation. By HUGH TAYLOR, Esq.* Communicated by the Author.

Up to a very recent period the attention of geologists has scarcely been directed to the facts which chemistry is capable of supplying in support of the doctrines of their science, although it admits of no doubt that the analysis of rocks and comparison of their chemical characters may in many instances materially contribute to the solution of its problems, or to the determination of disputed points. Of late, however, the chemical investigation of the volcanic rocks of Iceland and of some other districts has led to interesting results, and though the stratified rocks in some respects afford a less promising field of inquiry, much valuable information may be derived from a knowledge of their chemical composition.† In regard to this class of rocks, we are still almost entirely destitute of chemical information; and, under these circumstances, I venture to hope that the following analyses of the rocks of the coal formation may not be without value, in so far as, to the best of my knowledge, none of them—with the exception of coal itself—have yet been analysed. The analyses were made in the laboratory of Dr Anderson of Edinburgh, to whose kind assistance and encouragement I have been much indebted during their prosecution.

The rocks analysed were taken principally from Buddle's Hartley Colliery in the Newcastle coal field, and, though few in number, are calculated to throw more light on the general constitution of such rocks than might be at first sight expected, each specimen being selected as the type of a family, the members of which differ little in their physical or chemical properties; so much so, indeed, that no difficulty exists in referring any individual stratum to the family to which it belongs.

On examining the section of the coal field, a certain definite arrangement of the beds is apparent, and a tendency to the repetition of small groups of strata—each group consisting of the same, or nearly the same, series of rocks. The succession of the members of those groups is, of course, liable to a certain amount of variation, individual beds sometimes disappearing or "cropping out"—and so destroying the uniformity of the series; but it may be observed generally that each seam of coal is the centre of a group, and is enclosed above and below by a succession of strata more or less impregnated by bituminous or coaly matter, those in immediate contact with the

* Read to the British Association at Edinburgh, August 1850.

† We recommend to the particular attention of students of the chemistry of rocks the many valuable memoirs of Mr M. A. Delesse in the Annales des Mines, &c.; and also the important Lehrbuch der Chemischen und Physikalischen Geologie of the celebrated Dr Gustav Bischof.—*Edit. of Edin. Phil. Jour.*

coal partaking so much of its character as to be capable of undergoing combustion, which in receding from it they become gradually more and more earthy, until eventually all trace of organic matter is lost.

The succession of the rocks in each group, taking them in the most general point of view, is as follows, in descending order :—

Fire-clay or Thill.
Sandstone.
Blue shale.
Bituminous shale.
Coarse coal (sometimes Cannel.)
Coal.
Coarse coal.
Fire-clay.

This succession, as already observed, is not absolutely invariable, for even sandstone is occasionally found in contact with the coal, and other strata (as clay-ironstone for instance) are sometimes associated with the foregoing, whilst in other instances one or more members of the group are entirely wanting. But these are exceptional cases, the arrangement in the majority of instances being that which I have given above. The rocks employed for analysis were as far as possible selected from one group, and embrace all its characteristic members, so that from the analysis of these few substances we have a tolerably correct idea of the composition of the rocks of the whole formation.

The actual order of superposition of the specimens analysed is as follows :—The order is ascending, and figures are attached to the strata analysed.

		Feet.	In.	
1.	Fire clay (or Thill), .	2	0	in thickness.
	Coarse coal, . . .	0	7	"
2.	Good coal, . . .	5	2	"
3.	Coarse coal, . . .	0	3	"
4.	Bituminous shale, . .	0	2	"
5.	Blue shale, or slate-clay, .	3	1	"
6.	Micaceous sandstone, .	0	7	"
	Blue shale (again), .	0	10	"
	Sandstone (again), .	0	7	"
	Blue shale, inclosing nodules of ironstone, .	2	1	"
	Bituminous shale and coal,	0	6	"
	Ironstone with shells, .	1	11	"
7.	Muscle-bind, . . .	0	6	"

Before entering upon the details, it may be desirable to glance at the methods of analysis.

Carbon and *Hydrogen* were determined by combustion with chromate of lead, copper turnings being made use of for the deoxidation of any nitrous acid formed during the process. *Nitrogen* by Warrentrap and Wills' method. The *Sulphur* was determined by deflagrating the substance with pure nitrate of potash and carbonate of soda, precipitating with chloride of barium, and calculating the sulphur from the sulphate of baryta obtained. *Ash* of coals by complete ignition in a platinum crucible. The *Inorganic substances* were estimated in the hydrochloric acid solution. The portion not soluble in acids (or a substance in the first instance only slightly affected by them) was fused with a mixture of carbonates of potash and soda, and the analysis conducted in the usual manner. The alkalies were determined in some instances by fusion with carbonate of baryta, in others by attacking the substance with hydrofluoric acid. The carbonic acid was determined in the ordinary way. All the substances were dried at 212°; and in those which contained no organic matter the water of combination was determined by ignition.

The details of the analyses are as follows :—

No. 1. *Fire-clay.*—Sp. gr. 2·519, of a grey colour, streak dull, very soapy to the touch. It constitutes usually the basement of each coal seam. As that from "Buddle's Hartley" was not a fine specimen, one was taken from the base of the coal at Blaydon Burn Colliery in Tyneside, where it is made use of for the manufacture of fire-bricks, &c.

Composition.

Water of combination,	10·524
Lime,	·668
Magnesia,	·746
Peroxide of iron,	2·008
Alumina,	27·753
Potash,	2·189
Chloride of sodium and sulphate of soda,	·439
Silicic acid,	55·500
	99·827

No. 2. *Good Coal.*—Sp. gr. 1·259, fracture conchoidal. Interspersed rather abundantly with iron pyrites; depth from surface 64 fathoms; is from the "Low main seam" principally used for steam purposes, to which end it is largely exported.

Composition.

Carbon,	78·690
Hydrogen,	6·000
Nitrogen,	2·370
Oxygen,	10·068
Sulphur,	1·509
Ash,	1·363
	100·000

Ash of the above.

Peroxide of iron, . . .	14·237
Alumina, ...	10·883
Lime, ...	8·915
Magnesia, ...	1·010
Potash, ...	1·039
Chlorine, *traces.*	
Sulphuric acid, ...	8·210
Silicic acid, ...	53·151
Unburnt charcoal, . . .	26·57
	100·102

No. 3. *Coarse Coal.*—Sp. gr. 1·269 ; fracture slaty ; lies immediately over the good coal ; and the bed varies from two to six inches in thickness. It contains a large amount of iron pyrites, and is put to no useful purpose.

Composition.

Carbon, ...	70·307
Hydrogen, ...	4·714
Nitrogen, ...	1·446
Oxygen, ...	5·433
Sulphur, ...	1·236
Ash, ...	16·864
	100·000

Ash of the above.

Soluble in Acids.	Lime, . . .	1·286	
	Magnesia, . . .	0·420	
	Iron, . . .	2·187	
	Alumina, . . .	21·231	
	Potash, . . .	2·200	
	Soda, *traces.*		
	Sulphuric acid, . .	1·705	
	Silicic acid, . .	1·118	
			30·147
Soluble in Acids.	Lime, *traces.*		
	Magnesia, . . .	0·662	
	Iron, *traces.*		
	Alumina, . . .	6.530	
	Silicic acid, . .	60·812	
	Unburnt charcoal and loss,	1·849	
			69·853
			100·000

No. 4. *Bituminous Shale.*—Sp. gr. 1·860 ; rests on coarse coal ; two to three inches in thickness ; black, hard, and brittle, of a slaty structure ; contains impressions of the Flora of the period.

Composition.

Carbon,	26·700
Hydrogen,	2·630
Oxygen,	9·090
Nitrogen,	·934
Lime,	1·027
Magnesia,	·519
Protoxide of iron,	4·275
Alumina,	19·347
Potash,	·839
Soda,	·374
Chlorine, *traces.*	
Silicic acid,	34·276
	100·011

No. 5. *Blue Shale* (Slate-clay.)—Sp. gr. 2·536 ; in thickness about three feet ; of a bluish-grey colour ; is of a much more earthy appearance than last, on which it rests, is softer and not so slaty, and is studded with nodules of ironstone. Is nearly related to the fire-clay, both in composition and physical properties.

Composition.

Water of combination,	11·083
Lime,	0·595
Magnesia,	1·377
Peroxide of iron,	4·569
Protoxide of iron,	4·545
Alumina,	23·290
Potash,	2·089
Chloride of sodium, *traces.*	
Sulphuric acid, *traces.*	
Silicic acid,	52·452
	100·000

No. 6. *Micaceous Sandstone.*—Sp. gr. 2·598, over last, varying from six inches to six feet in thickness; of a fine white colour, close-grained, and with small plates of mica, very conspicuously seen.

Composition.

Water of combination,	6·888
Peroxide of iron,	9·539
Alumina,	8·126
Lime,	1·112
Magnesia,	0·325
Potash,	1·655
Soda,	1·859
Silicic acid,	70·257
	99·761

No. 7. *Muscle-Bind.*—Sp. gr. 2·592; in thickness, six inches; is a clay-ironstone thickly imbedded with indurated shells of a brown colour and very brittle. This bed resembles the muscle-bind of the Derbyshire and Yorkshire coal-fields; and is found also in the coal-fields of Scotland. It lies in this instance at the depth of 62 fathoms from the surface, or 9½ feet above the "Low-main coal."

Composition.

Soluble in Acids.	Organic matter and water of combination,	11·221	
	Protoxide of iron,	18·637	
	Manganese, *traces.*		
	Alumina,	1·194	
	Lime,	4·084	
	Magnesia,	1·078	
	Chloride of sodium, *traces.*		
	Potash,	1·319	
	Silicic acid, *traces.*		
	Carbonic acid,	14·057	
			51·590
Insoluble in Acids.	Oxide of iron, *traces.*		
	Alumina,	16·292	
	Lime,	0·988	
	Magnesia,	0·288	
	Silicic acid,	31·068	
			48·636
			100·226

No. 8. *Cannel Coal.*—Sp. gr. 1·319:—is a black, homogeneous mass, hard, brittle, and capable of taking a fine polish, fracture conchoidal. Though not in the section of "Buddle's Hartley," is often found in connection with the coal, as roof, base, or even interstratified with it; that analysed is a fine specimen from "Blaydon Main" colliery, in Tyneside.

Composition.

Carbon,	78·056
Hydrogen,	5·805
Nitrogen,	1·854
Oxygen,	3·119
Sulphur,	2·223
Ash,	8·943
	100·000

A comparative view of these analyses leads to some interesting results, and appears to indicate a pretty close connection between some of the members of the coal formation.

Comparing the organic constituents of the different coals and bituminous shale, it will be observed that they so far resemble one another, as all to contain carbon, hydrogen, nitrogen, and oxygen, and a general similarity is apparent in the quantitative relations of these elements. But this relation is to a great extent concealed by the variable proportion of inorganic matter which the substances contain, and becomes much more striking when the ash is subtracted, and the composition of the organic part calculated on 100 parts. When this is done, we obtain the following numbers:—

	Coal.	Coarse Coal.	Cannel Coal.	Bituminous Shale.
Carbon,	81·01	85·83	87·86	67·84
Hydrogen,	6·17	5·75	6·53	6·68
Nitrogen,	2·44	1·76	2·09	2·37
Oxygen,	10·38	6·66	2·53	23·11
	100·00	100·00	100·00	100·00

From these numbers it appears that all these substances present a pretty close resemblance, except the organic matter of the bituminous shale, which is very much richer in oxygen than any of the others. This difference, however, is more apparent than real, and I believe it to depend upon the method of analysis employed, by which any water which may exist in combination with the inorganic constituents, comes to be determined along with, and reckoned as part of, the organic matter. I shall have occasion, in discussing the inorganic constituents of these rocks, to shew that the inorganic part of the bituminous shale does in all probability contain water, the exact amount of which cannot, in presence of organic matter, be determined by analysis, but which, from other considerations, would appear to amount to about 6·6 per cent. If we therefore subtract from the organic matter of the shale the quantities of hydrogen and oxygen corresponding to 6·6 per cent. of water, and then calculate the result upon one hundred parts, we obtain the following numbers, which agree in a remarkable manner with those obtained from the good coal.

Carbon,	81·62
Hydrogen,	5·78
Nitrogen,	2·84
Oxygen,	9·76
	100·00

These analyses may then be considered as establishing the fact, that the organic matter which permeates the strata of the coal-formation is chemically identical with coal itself; in fact, that bituminous shale differs in no respect from coal, except in containing a largely preponderating amount of ash, and that whatever may have been the manner in which coal has been formed, bituminous shale must be produced under precisely similar circumstances. The analyses appear, however, further to indicate the existence in the coal-field of vegetable matter in two different phases of decomposition, for the cannel and coarse coals contain a very much larger amount of carbon, and smaller of oxygen, than the other two, which approximate very closely to one another in composition. Now the gradual decomposition of vegetable matter is attended by the gradual diminution of the oxygen present, carbonic acid, which for every six parts of carbon carries off 22 of oxygen, being gradually evolved, and it is fair to admit that the composition of the good coal and organic matter of the bituminous shale, indicates a less advanced state of decomposition than that of the other two; and as it must be manifest that all are exposed to the same causes of decomposition now, the difference which is observed must be due to causes operating before the deposition of the superincumbent strata.

The analyses of the inorganic constituents of the different rocks also afford evidence of a certain connection among the different members of the formation, though it is less obvious both on account of their greater complexity and the certainty of their being, in some instances, mere mechanical mixtures, liable to great variations. If, however, we compare only that portion of certain of these substances which is insoluble in acids, and which analysis shows to be of constant composition, we arrive at some interesting conclusions. The whole of the fire-clay, with the exception of small quantities of lime and potash, is insoluble in acids. Now, the clays already known to us, are definite mineral compounds, presenting an invariable composition. Porcelain-clay, for instance, is a hydrated sesquisilicate of alumina represented by the formula $2Al_2 O_3\ 3Si\ O_3 + 3HO$, and the same composition is found in all our purer clays, and the fire-clay of the coal formation is also a definite silicate, though not identical with porcelain-clay, its composition, setting aside as non-essential the small quantities of lime and potash, corresponding very closely with the formula $Al_2\ O_3\ 2Si\ O_3 + 2HO$. Now, the same clay can be traced in other members of the coal formation; thus, the muscle-bind is a mixture of this silicate with carbonates of iron and lime. This connection is not seen as the analysis stands, but becomes apparent by comparing the fire-clay, calculated as anhydrous, and the insoluble matter of the muscle-bind calculated to 100 parts.

	Fire-clay.	Insoluble matter of muscle-bind.
Silicic acid,	62·14	63·89
Alumina,	31·07	33·49
Peroxide of iron,	2·24	...
Lime,	0·74	2·01
Magnesia,	0·83	0·61
Potash,	2·45	...
	100·00	100·00

Here the sole difference appears to be that, in the fire-clay, a small quantity of alumina is replaced by the isomorphous peroxide of iron, and both lead to the same formula $Al_2\ O_3\ 2Si\ O_3$. This represents both substances as anhydrous, but the fire-clay contains water in combination, corresponding, as before observed, to two equivalents, and it admits of no doubt that the insoluble matter of the muscle-bind, as it exists in the rock, is also hydrated, although, from the presence of organic matter, it is impossible to determine the amount of water in an accurate manner. An attempt was however made to determine the quantity of water by gently heating the substance so as to avoid decomposing the organic matter; and in this way 3·08 per cent. of water was obtained, but obviously no reliance can be placed upon this as a quantitative determination, because it was impossible to obtain a sufficient heat to expel the whole of the water without destroying the organic matter, but it is sufficient to establish the fact that water actually was present.

The blue shale and inorganic matter of the bituminous shale, form another pair very similar to one another, though different from the fire-clay. In order to render this similarity apparent, I give the results of the analyses calculated the blue shale as anhydrous, and the inorganic matter of the bituminous shale in 100 parts.

	Blue shale.	Bituminous shale.
Silicic acid,	58·99	56·51
Alumina,	26·19	31·89
Peroxide of iron,	5·14	...
Protoxide of iron,	5·11	7 04
Lime,	0·67	1·69
Magnesia,	1·54	0·85
Potash,	2·34	1·38
Soda,	...	0·61
	100·00	100·00

In these two substances, in addition to silicic acid and alumina, we have a large amount of bases with one equivalent of oxygen, among which protoxide of iron and potash predominate. The relation of silica to the sesquiatomic bases is the same in both, alumina in the blue shale, being in part replaced by peroxide of iron, and though the quantity of monatomic bases in the bituminous shale considerably exceeds that in the blue shale, still the similarity of the two is sufficiently striking. It will be observed that, in the above calculation, both substances are represented as anhydrous, while the blue shale actually contains 11 per cent. of water. I infer however from the analogy of the two substances, that the inorganic matter of the bituminous shale must also be a hydrated compound, containing the same proportion of water which I have accordingly supposed it to be in the calculation of the constitution of its organic part, given in page 147.

The inorganic matter of the other rocks is too various to admit of any conclusions being drawn regarding their constitution, although they present some points of interest, which, however, I shall not at present attempt to discuss.

In conclusion, I may be permitted to hope, that the results of these analyses are sufficiently interesting to lead others into the same field of inquiry, and that from the accumulation of facts, we may eventually be led to some general laws regulating the deposition of the stratified rocks.

On the Tetramorphism of Carbon. By HENRY CLIFTON SORBY, Esq., F.G.S. Communicated by the Author.*

The four species into which I find it necessary to divide carbon, may be characterised as diamond, graphite, hard coke, and anthracite. They have long been distinguished practically; the object of the present paper is to explain their various crystalline forms and relationships.

To commence with the most dense, we have first of all diamond, which has long been known to be crystallized in the regular system, cleavable parallel to the planes of the octahedron, and to have a specific gravity of about 3·52.

Secondly, we have graphite, plumbago or black lead, which, though usually amorphous, is sometimes found crystallized in regular six-sided prisms, cleavable parallel to the terminal planes. This form of carbon often contains much impurity, and its specific gravity is thereby rendered variable, but when due allowance is made for the ashes, as I shall subsequently describe, it is about 2·18.

But, besides these, I find there are two other species, viz. :—hard coke and anthracite. Hard coke, like diamond, belongs to the re-

---

* Read before the British Association at Edinburgh, August 1850.

gular system, but I shall shew that its primary form is not an octahedron but a cube, and that it has very different properties and specific gravity, viz., about 1·89. Anthracite, however, is crystallized in the square prismatic system, and has a specific gravity of about 1·76.

The manner in which I have been able to ascertain the crystalline form of these bodies is as follows :—I bruise a small portion to a fine powder in a mortar along with some ten times the quantity of soft chalk, which is employed to protect the fine fragments from injury after being detached, and afterwards dissolve the chalk in acid, and wash the powder left by subsidence and decantation. Then spreading a portion with the aid of a little water on a piece of thin glass used for microscopic object covers, and drying it, I examine it through the glass with a magnifying power of about 400 linear. By this treatment, the crystalline faces often arrange themselves on the surface of the glass, as can be told by the adjustment of the focus, though many do not ; and, of course, the angles which are then seen are of no value for measurement. Their magnitude is determined by the doubly refracting goniometer, invented by Dr Leeson, by the use of which beautifully contrived instrument, with proper precautions, the angles of fragments of $\frac{1}{4000}$th of an inch in diameter, or even less, can be ascertained within a small fraction of a degree. Of course by this method we can only measure the angles which occur on one plane, and the size of the particles makes it impossible to turn them round. I have, therefore, been compelled to adopt a very different method of procedure to that usually followed in crystallographic researches ; and after measuring a number of angles, sometimes connected, and at others detached, I have, by comparing them by calculation, been able to build up the primary form from which they are derived.

It is not every specimen of hard coke which shews the crystalline structure well. That which is highly vesicular does not exhibit it at all, nor does that which is smooth and globular, which presents a similar appearance when highly magnified. That which is dense and has a highly metallic lustre is the best, though even with it not many particles with their angles good and well placed for measurement are found, as is indeed to be expected from the nature of the substance and the treatment to which it is necessary to subject it. Moreover, I should say that it does not cleave readily, and there are many particles which are obviously broke in a splintery manner, quite independent of structure, but these angles can generally be easily distinguished from those due to crystalline cleavage. Notwithstanding all these necessary difficulties, by very patient examination I have found amply sufficient good angles, well placed for measurement, to satisfy me that it is crystallized in the regular system. The angles are of 90°, 45°, 60°, 30°, 70½°, 109½°, or as near those values as it is possible to ascertain, and I should say that they most certainly do not differ from them by more than ¼°, or at most ½°, and that it is much more probable that they really are of those values, though the nature of the case prevents me from being so sure as when tangible crystals can be measured by the reflecting goniometer. These angles will at once be seen to be such as are derivable in the simplest manner from the regular system; and the way in which they are associated fully agrees with that supposition.

In a similar manner I find that anthracite is crystallized in the square prismatic system, the axes having the relative values of 5, 5, and 3. It cleaves much more readily than coke, and many more good angles can be found than in it. It is, however, very much more complicated in its structure, for not only does it cleave with relation to the axes 5, 5, and 3, but also to four times the axis 3; and owing to this property so great a number of angles are found as to make it somewhat confusing. The anthracite, which I have chiefly used in my experiments, is a very good specimen of American, and it cleaves best through the extremities of the axes 5, 5, and 3, though also with relation to four times 3, as can frequently be seen from the same fragment. Some other specimens which I have examined appear to cleave more readily in the latter direction. To explain the manner of derivation of the various angles which occur would occupy more space than it would deserve. I shall, therefore, merely give their approximate values, and content myself with stating that, both in magnitude and arrangement, they are all readily derivable by various most simple cleavages from a square prism having its axes of the relative values of 5, 5, and 3. To show how closely most careful measurements agree with this supposition, I will give two examples,—observations 99° 24′, also 106° 21′; theory 99° 22′, and 106° 25′.

The following are among the most common angles which occur, $80\frac{1}{2}$°, $99\frac{1}{2}$°, $40\frac{1}{4}$°, $49\frac{3}{4}$°, 90°, 45°, $52\frac{3}{4}$°, $74\frac{1}{2}$°, 62°, 118°, 31°, 59°, which are related to the axes 5, 5, and 3; also $22\frac{1}{2}$°, $67\frac{1}{2}$°, $16\frac{1}{2}$°, $73\frac{1}{2}$°, which are related to the axes 5, 5, and four times 3. We have by various intersections others, but it is needless to particularise them.

Although charcoal, from its nature, is ill fitted for such an examination, yet, by proceeding in the manner above described, I have found in it angles which agree with those of anthracite as closely as could be measured, but only in particles of about $\frac{1}{10,000}$th of an inch in diameter. Lamp-black presents merely a granular appearance, as though consisting of clusters of particles of about $\frac{1}{100,000}$th of an inch in diameter, and no trace of crystals. The same may be said of gas carbon which, however, is chiefly of the coke form, as is shewn by its specific gravity and metallic lustre.

Whence it should appear that we have carbon crystallized in three different systems—the regular, rhombohedric, and the square prismatic; and what is remarkable, it occurs in two distinct conditions with different volumes and properties in the same system, the regular, viz., as diamond and coke, though their primary forms are not the same, making therefore in all four crystalline species, the mutual relationship of which I will now endeavour to prove to be very simple. These primary forms are shewn in figs. 1, 2, 3, and 4, which represent the relative volumes of equal weights of the four species.

To ascertain the specific gravity of graphite, hard coke, anthracite, or charcoal with sufficient accuracy, I have found it absolutely necessary to have them in the state of extremely fine powder; and, in the case of charcoal, to boil it first in acid to dissolve the glaze of ashes. I then heat it to dull redness, without access of air, to dispel any volatile matter which it may contain; and afterwards well boil it in water in a glass bulb, and fill it full of boiled water and cork it. By this means, the water, having been deprived of air, dissolves when cold any that may remain amongst the powder. It is then weighed in water, and the weight in air is ascertained by subsequently drying it in the bulb. The powder is then burned to ashes, and their weight in air and water ascertained, which being deducted respectively from those of the substance, we can calculate its specific gravity free from the effects of the ashes. The following tables exhibit in column A the specific gravity of the substance, no allowance being made for the ashes, in B the amount per cent. of ashes, in C their specific gravity, and in D the specific gravity of the substance corrected from the effects of the ashes:—

	A	B	C	D
Graphite,	2·276	3·	4·4	2·244
...	2·323	22·	3·0	2·188
...	2·378	29	3·1	2·177
...	2·247	12·	3·0	2·173
...	2·237	13·	3·2	2·159
...	2·200	13	3·2	2·122

The mean of all these is 2·177. It will be observed that the first and last experiments differ considerably from the others, which I have some reason to believe is partly due to errors of experiment. The mean of the others is 2·174. If, however, we take 2·177 for the specific gravity of pure graphite, I think we shall not be far wrong.

	A	B	C	D
Hard coke,	1·923	5·7	2·6	1·890
...	1·905	3·	2·5	1·891
...	1·902	2·1	2·5	1·892
...	1·900	1·8	2·4	1·893

The mean of these, which agree very closely with one another, is 1·8915.

	A	B	C	D
Anthracite,	1·800	7·5	2·5	1·760
...	1·800			1·760
...	1·800			
Charcoal,	1·789	·9	2·5	1·784
...	1·824	·9	3·0	1·824
Lamp-black,	1·780	1·0	2·5	1·774
...	1·792			

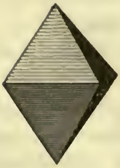
Whence the mean specific gravity of anthracite may be taken to be about 1·760. In the case of charcoal and lamp-black, the latter experiment in both was with the substance not first heated to dull redness, but the first in each case was most carefully conducted, and I think it is more correct than a mean of the two observations would be. I therefore conclude that the specific gravity of charcoal is very near 1·784, and lamp-black 1·774, which it will be seen to agree very closely with one another, and with that of anthracite, but differ very considerably from that of coke. The mean of the three is 1·773.

The present may perhaps be as proper a place as any for me to state my reasons for thinking that anthracite, charcoal, and lamp-black are one species. I certainly will admit that in some cases what is not genuine anthracite is so called; but that which I have experimented on not only has the same specific gravity, as I have shewn above, but in all other properties agrees most closely with charcoal and lamp-black, whilst they all differ very considerably from coke, as I shall subsequently shew. Moreover, as I remarked, the angles found in very minute particles of charcoal appear to be the same as those in anthracite, though they are so very minute that I will not insist on their correct measurement. Taking these facts into consideration, I think it is very highly probable that the square prismatic crystallization found in anthracite is really that which belongs to these three varieties.

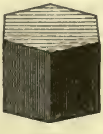
I have not examined the specific gravity of diamond, but the mean of many statements which I find recorded is 3·521.

By comparing the above specific gravities with the crystalline forms of the various species, I have come to the conclusion that they all result from atoms having the relative volumes of $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$, arranged in the manner indicated by their respective forms and cleavages.

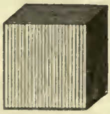
1



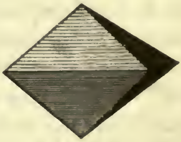
2



3

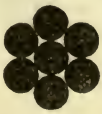


4



5

6



7

8

We will begin our calculations with the mean specific gravity of the square prismatic form, which is 1·773. Considering the relative frequency of the angles found in anthracite, I should say that its best cleavage is parallel to the faces of the obtuse octahedron, having its axes as 5, 5, and 3. Diamond also cleaves parallel to the planes of the regular octahedron, and therefore it is most probable that the atoms in both cases are arranged in a similar manner, viz., in rows parallel to the faces of the octahedrons, so that their mutual arrangement on those faces would be as shewn by figs. 5 and 8, only in the diamond being spheres, and in the square prismatic form spheriods with their axes in the ratio of 3 to 5; the former being the axis of rotation of the ellipse generating it, which would be placed in the crystal parallel to the axis of the prism; and, therefore, the section parallel to the faces of the octahedron would not exhibit ellipses with axes of those relative values, but as shewn by fig. 8.

Now, I have found by experiment, that the specific gravity of the square prismatic form is 1·773, and this multiplied by 2 is 3·546. The mean specific gravity of diamond is 3·521, which differs from that deduced above by only $\frac{1}{140}$th, which is much less than what occurs in the statements of different experimenters. Whence I think we may conclude that the atoms of the square prismatic form and diamond are, to one another in their relative volume, as $\frac{1}{2}$ to $\frac{1}{4}$.

Again, supposing the atoms of coke to be spheres arranged cubically, so that on each face they would be related to one another in the manner shewn by fig. 7, and those of diamond to be as above described, and also that their relative volumes are as $\frac{1}{3}$ to $\frac{1}{4}$, it is easily shewn mathematically, that their relative specific gravities would be as 3 to $4\sqrt{2}$. Calculating on these suppositions, and taking the specific gravity of diamond at that above calculated from that of the square prismatic form, the specific gravity of coke would be 1·880, which differs from 1·891, found by experiment, by only $\frac{1}{180}$th. Whence the volume of the atoms of coke is to that of those of diamond as $\frac{1}{3}$ to $\frac{1}{4}$, and to that of those of the square prismatic form, as $\frac{1}{3}$ to $\frac{1}{2}$.

Again, supposing the atoms of graphite to be either spheres or ellipsoids, having the same volume as the spheres in coke, but arranged as a regular six-sided prism, if ellipsoids with their axes parallel to the prism, that is to say, hexagonally parallel to the terminal planes, as shewn by fig. 6, and rectangularly in the direction perpendicular to them, which structure agrees perfectly with the fact of its cleaving only parallel to the terminal planes, it is easily shewn mathematically, that their relative specific gravity would be as 2 to $\sqrt{3}$. Calculating on these suppositions, and taking the specific gravity of coke at that calculated from that of the square prismatic form, the specific gravity of graphite would be 2·172. The mean found by experiment, as given above, is 2·177, which differs from that calculated by only $\frac{1}{400}$th.

All these calculated results, and the mean specific gravities found by experiment, will be more readily compared by inspecting the table given farther on, and it will be seen that they agree so very closely, that I think there can be no doubt of the relative volumes and arrangement of the atoms being as above described. Whence, I think, I am safe in concluding that *the four species of carbon are brought about by its existing in atoms of the relative volumes of* $\frac{1}{2}$, $\frac{1}{3}$, *and* $\frac{1}{4}$, *of the shape, and arranged in the manner indicated by their respective crystalline forms.* I would remark, that the extreme simplicity of these relations, and the close agreement of observation and theory, prove most satisfactorily the truth of the usual supposition, that *the atoms of bodies crystallized in the regular system, are spheres, and in the square prismatic, spheroids;* for unless they be assumed so to be, their relationships would not be as above described. Moreover, it is easily shewn mathematically, that the spheroids of the square prismatic system must be generated by the revolution of an *ellipse*, for no other simple form would answer the conditions of the problem.

It must also be borne in mind, that in these calculations I have assumed, that the spherical or ellipsoidical atoms really *touch* one another. This is of course a point about which there may be difference of opinion, but whether we consider them to do so or no, it will be seen that this assumption agrees remarkably well with experiment. At all events, if the atoms be supposed to have variable atmospheres of heat and electricity, these atmospheres would appear to possess the same properties as rigid spheres or ellipsoids. Whether the difference of volume be attributable to the variation in magnitude of these atmospheres, or to that of the elementary atoms themselves, can only be a matter of conjecture. I have therefore in the present paper used the term atom for those ultimate spherical or ellipsoidical particles, the difference in volume, form, and arrangement of which, in my opinion, produce the different properties of the four species of carbon.

I would also call attention to the necessity of making a most marked distinction between the arrangement of the spherical atoms as an octahedron and a cube, for, though both coke and diamond are crystallized in the regular system, their difference is such that we cannot say that they exist in the same form, the former being a cube and the latter a regular octahedron.

It has been usual to consider graphite to be crystallized in the rhombohedric system. I however think that its structure agrees much better with the supposition of its atoms being arranged as a regular six-sided prism, and the close agreement of the calculated and found specific gravity proves to my mind that I am correct in so thinking. I would therefore call attention to the necessity of making a wide distinction between its primary form and a rhomb, although undoubtedly the regular six-sided prism is, in many cases, a secondary form of the rhombohedric system.

One most important circumstance connected with the above expressed views is the very interesting connection which exists between the volume of the atoms and specific heat of the four species. It has long been known that the specific heats of the different forms of carbon vary very much, and exhibits a simple relation to one another, but, so far as I am aware, their true connection has been overlooked. Gmelin, in his Hand-book of Chemistry (*Cavendish Society's Translation*, vol. i., page 244), when alluding to the relation between the equivalents of elementary bodies and their specific heat, says, " The capacity for heat of carbon in the form of diamond is $\frac{1}{4}$, in that of graphite $\frac{1}{3}$, and in that of charcoal $\frac{1}{2}$, its ordinary amount. These exceptions cannot be explained away; we cannot treble, quadruple, nor even double the atomic weight of carbon without incurring great inconveniences." I however advance the following general law as regards this element :—*The capacity for heat of the atoms of carbon when of different volumes varies directly as their magnitude, and does not therefore bear merely a simple relation to the equivalent.* Of course it is well known that the statements of the specific heat of bodies given by different authors vary considerably, as might be expected from the nature of the experiments. If, however, we take the mean of those for the various forms of carbon given in the table at page 241 of Gmelin's Hand-book of Chemistry, we shall find that they agree very nearly with the above statement. The comparison of these means with theory will be seen from the table given farther on, and the existence of the above propounded relation between the volume of the atoms and their capacity for heat will readily be seen; and this fact, coupled with the close agreement of the specific gravities, I think, proves that the suppositions which have elicited it are correct.

This relation most probably holds good for all elementary bodies, and if we suppose it to do so, it will, I think, throw some light on the connection between their equivalents and specific heats. It is well known that, in the majority of elements, the equivalent multiplied by the specific heat is a constant quantity. Since, however, as I have shewn above, the capacity for heat varies as the volume of the atoms, it would be necessary for them all to exist in the same relative volumes for this law to hold good universally. The deviations from it, I think fully agree with this supposition. The chief of these are carbon, which, as above described, exists with three different volumes and specific heats, none of which are normal;—oxygen, which has an abnormal volume and specific heat of $\frac{1}{2}$;—iodine, phosphorus, arsenic, antimony, silver, and gold, which, to judge from their specific heat, have an abnormal volume of 2, and bromine of 3. If the above views be adopted, it will not be necessary to alter their equivalents, as has been suggested by some chemists.

I now subjoin a table of the specific gravities, specific heats, and the various distinctive properties of the four species of carbon.

Name of Species.	Anthracite.	Hard Coke.	Graphite.	Diamond.
System of crystallization,	Square prismatic.	Regular.	Rhombohedric?	Regular.
Primary form of crystal,	Obtuse octahedron.	Cube.	Regular hexagonal prism.	Regular octahedron.
Form of the ultimate atoms,	Ellipsoids with axes as 5, 5, & 3.	Spheres.	Spheres or ellipsoids.	Spheres.
Specific gravity, theory, relative,	$2 \times \sqrt{2}$	3×1	$3 \times \frac{2}{\sqrt{3}}$	$4 \times \sqrt{2}$
Specific gravity, theory, water = 1,	1·773	1·880	2·172	3·546
Specific gravity, experiment,	1·773	1·891	2·177	3·521
Comparison of theory and experiment,	Assumed equal.	$\frac{1}{180}$ too little.	$\frac{1}{440}$ too little.	$\frac{1}{140}$ too much.
Relative volume of the atoms,	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{4}$
Specific heat, theory, relative,	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{4}$
Specific heat, theory, water = 1,	·2714	·1809	·1809	·1357
Specific heat, experiment,	·2714	·2028	·2019	·1331
Comparison of theory and experiment,	Assumed equal.	$\frac{1}{9}$ too little.	$\frac{1}{9}$ too little.	$\frac{1}{50}$ too much.
Conducts electricity,	Tolerably well.	Extremely well.	Extremely well.	Not at all.
Optical properties,	Black and opaque.	Opaque with a metallic lustre.	Opaque with a metallic lustre.	Colourless and transparent.
Hardness of Mohs' scale,	2· to 2·5	8·5	About 1·	10·

By comparing the facts tabulated above, it will readily be seen that these four species differ in properties in a very decided manner; and the comparison of these differences with the crystalline form and volume of the atoms, appears to lead to some interesting results. In the first place, it is obvious that the properties of the different species must depend either on the different forms and volumes of the atoms, on their manner of arrangement, or on both. I have shewn that the specific gravity is a function of both, and the omission of the correction required for the difference of arrangement appears to me to be one of the chief reasons for the difficulty hitherto experienced in attempting to arrive at a correct theory of the relative volumes of solid bodies, as I hope on another occasion to shew. This will more readily be seen by inspecting the upper row of figures in the table, representing the relative specific gravities, in which the figure on the left hand of the × is the inverse volume of the atoms, and that on the right the variation produced by their difference of arrangement; and if it were omitted, we should obviously have no simple relation between the volumes, and the connection of it with the specific heat would not be at all simple. It will, however, readily be seen that the specific heat is a simple function of the volume of the atoms, and in no way related to the space between them, which depends upon the crystalline form. Such would also appear to be the case with the power of conducting electricity, or transmitting and reflecting light, for these properties are the same when the volume of the atoms is so, though the crystalline forms differ, and differ when it differs. The hardness, however, seems to be a compound function both of their volume and arrangement.

It has long been known that diamond, when subjected to intense heat, increases in volume, becomes black, and a conductor of electricity. I do not know whether it then takes the form of anthracite or coke, but conjecture that it may become pseudomorphous coke, such as the square prismatic species is converted into by a high temperature.

When anthracite, charcoal, or lamp-black have been heated only to dull redness, though they conduct electricity, they do so very indifferently compared with coke or graphite; and their hardness is then very much less than that of coke. If, however, they are heated to a bright red or white heat, they conduct electricity vastly better than before, and anthracite then cuts glass in the same manner as coke was shewn to do by my esteemed relative Mr James Nasmyth. In the case of charcoal, De la Rive and Marcet have shewn that this treatment causes the specific heat to decrease from ·2964 to ·2009, which is from that of the square prismatic form to that of coke, or nearly so. When examined by the microscope, anthracite nevertheless still presents the same crystalline structure as before, and its specific gravity, as determined from fine powder, is also the same. I therefore think that a high temperature converts the ultimate atoms of the square prismatic species into those of the coke form, though the crystalline structure is not changed, and it must therefore be considered as pseudomorphous.

When pieces of coke are used for the electric light, that piece which is attached to the anode is softened and partially converted into graphite, whilst that at the cathode is unaltered. My attention was first called to this fact by my friend Mr James Haywood. I found that the specific gravity of a portion thus treated was 1·99 (correct for ashes), which agrees with the supposition of its consisting of about $\frac{1}{3}$ graphite. Some parts were undoubtedly more perfectly converted into it than the rest, and especially the points from which the electricity had passed, but the appearance of the whole taken together, and its behaviour when pounded, agreed very well with the above deduction. It would therefore seem that this high temperature and positively electrical state cause the atoms to lose their former polarity, and to take others not having nearly so great an intensity of action, and thus rendering graphite much softer than coke.

Judging from these and other well-known facts, I am of opinion that diamond has been formed at a comparatively low temperature, the square prismatic species at about a low red heat, and coke at a higher temperature. The fact of the portion of coke which is attached to the anode being changed into graphite, and not that at the cathode, indicates, I think, that the change is due, not so much to the mere effect of heat, as to the combined action of heat and of a highly positively electrical state.

Although it may perhaps be too great a refinement, considering circumstances, I would remark that the fact of the specific gravity of diamond being less than theory indicates, whilst those of coke and graphite are greater, agrees with the above expressed view of it having been formed at a lower, and them at a higher temperature than the square prismatic species, with which they are compared in the table.

On the alleged evidence for a Physical Connection between Stars forming Binary or Multiple Groups, deduced from the Doctrine of Chances. By JAMES D. FORBES, F.R.S., Corr. Member of the Institute of France, &c. Communicated by the Author.

An opinion has long obtained amongst astronomers that the great number of cases which occur in the heavens of two or more stars being apparently very close to one another (constituting what have been called double, triple, or multiple stars), constitutes of itself an argument for a more than *apparent* connection between the members of those groups.

It is evident that two stars may constitute an *apparently* double star without any real proximity between them, merely because a line passing through the eye of the spectator and the nearer star may, if prolonged into space (no matter how far), pass somewhere near a second star, whose position would therefore seem almost to coincide with the first, although the distance which separates them might be indefinitely great. Such stars are sometimes said to be "optically" double. On the other hand, it may happen that the two stars are really as well as seemingly near, and may act upon one another by their mutual attractions, after the manner of sun and planet. Such stars are called "physically" double.

Nearly a century ago the Rev. John Mitchell attempted to deduce from the theory of probabilities, the chances against the fortuitous approximation of two or more stars, supposing the stars generally to be "scattered by mere chance as it might happen." He concludes that there is a probability of 80 to 1 that the two stars β Capricorni are physically connected, and above 500,000 to 1 that the stars of the Pleiades are so. These results have been implicitly adopted by most subsequent writers on probabilities and on astronomy.

The author denies *in toto* the legitimacy of the influences, and the possibility of putting a numerical value upon such evidences of physical relation. As *inductive presumptions* of such a connection he admits that they have a certain evidence in their favour; but one not more expressible by numbers than that of any physical theory, such as that of gravity. The author endeavours to show that Mitchell has confounded the mere *expectation* of an event which may or may not occur, with the inherent probability that a particular event which *has* occurred, should happen rather than any other possible event. He also shows that Mitchell's mathematical expression of the result of random scattering leads to absurd results, and must therefore be erroneous and delusive.

The following was stated to the meeting as the results at which the author had at that time arrived :—

(1.) The fundamental principle of Mitchell is erroneous.

The probability expressed by it is an altogether different probability from what he asserts. His calculations are also apparently inaccurate, in some instances at least.

(2.) All the *numerical* deductions of his successors are equally baseless.

(3.) Were Mitchell's principle just, a perfectly uniform and symmetrical disposition of the stars over the sky would (if possible) be that which could alone afford no evidence of causation, or any interference with the laws of "random;"—a result palpably absurd.

(4.) Special collocations, whether (α) distinguished by their symmetry, or (β) distinguished by an excessive crowding together of stars, or the reverse, inevitably force on the reasoning mind a more or less vague impression of causation; —an impression necessarily vague, having nothing absolute, but depending on the previous knowledge and habits of thought of the individual, therefore incapable of being made the subject of exact (*i. e.* mathematical) reasoning.

Probable Effects of Vegetation on Climate.

Meteorologists throughout India had for some time been engaged in examining the probable effects of vegetation and of moisture on climate, and the following results, mainly from an able paper by Dr Balfour of Bombay, and the Edinburgh New Philosophical Journal, gives a general outline of the present state of our knowledge on this subject.

The fall of rain at the elevations of 2000 to 4500 feet, on ridges exposed to currents of wind from the sea, amounts to about 200 inches annually; it decreases, both as we ascend and as we proceed into the interior. Along the shores of Hindoostan it averages betwixt sixty and eighty. On the tolerably fertile or rarely wooded portions of the great plateau, it amounts to betwixt twenty and thirty-five. At Bellary, it averages from ten to fifteen; and when we inquire the cause of this sudden diminishment, we find that the districts around are destitute of trees, and nearly devoid of all sorts of moisture or local vegetation. Humboldt, in noticing the barrenness and extreme aridity of the vast plains approaching the Orinocco from the Andes, lat. 9°, states, that the people assured him that the fall of rain had diminished within the last century, and that since the Spanish conquest, the trees, formerly abounding, have been destroyed. The earlier settlers in Carracas are well known to have destroyed the climate by removing the trees; rain formerly abounded where now there is none. "By felling the trees," says Humboldt, as quoted by Balfour, "that cover the tops and sides of the mountains, men in every climate prepare at once two calamities for future generations; the want of fuel, and a scarcity of water. Trees, by the nature of their perspiration and the radiation from their leaves in a sky without clouds, surround themselves with an atmosphere constantly cold and misty. They affect the copiousness of springs, not, as was long believed, by a peculiar attraction for the vapours diffused through the air, but because, by sheltering the soil from the direct action of the sun, they diminish the evaporation of the water produced by rain. When forests are destroyed, as they are everywhere in America, by the European planters with an imprudent precipitation, the springs are entirely dried up or become less abundant. The beds of the rivers, remaining dry during a part of the year, are converted into torrents whenever great rains fall on the heights. The sward and moss disappearing with the brushwood from the sides of the mountain, the waters falling in rain, are no longer impeded in their course; and instead of slowly augmenting the level of the rivers by progressive filtration, they furrow during heavy showers the sides of the hills, bear down the loosened soil, and form those sudden inundations that devastate the country. Hence it results that the destructions of forests, the want of permanent springs, and the existence of torrents, are three phenomena closely connected together." Dr Duncan, of the Bombay medical establishment, mentions that, within his own experience, the climate at Dapoolie had been much more hot and dry—streams now dry up in December which used to flow till April or May; and this he attributes to the destruction of trees which formerly clothed the hills, now left barren and desolate by their removal. In the Southern Concan, within the space of fifteen years, the climate has been greatly deteriorated by the diminution of vegetation, and consequently, of rain. The people of Pinang have memorialized Government against the destruction of their forests, sure that the result by its continuance will be the ruin of the climate. The dreadful droughts which now so frequently visit the Cape de Verd Islands, is avowedly due to the removal of their forests; in the high lands of Greece, where trees have been cut down, springs have disappeared. The excessive rains around Rio Janeiro, have been modified and reduced by the diminution of the woods. The valley of Aragua, in South America, affords a curious series of examples of diminution of rain by the destruction of trees, and increase of fall by their multiplication. The valley is completely enclosed by high ranges of hills, giving rise to various streams and rivulets, the waters of which form a lake at the extreme end of the valley. The lake has no exit, and its superfluous waters are carried off by evaporation. Betwixt 1555, when it was described by Oviedo, and 1800, when it was visited by Humboldt, the lake had sunk 5 or 6 feet, and had receded several miles from its former shores, the portion of the basin thus left dry appearing the most fertile land in the neighbourhood. This is ascribed by the distinguished traveller just named, to the destruction of the trees on the mountains. When the war of liberation broke out, agriculture was neglected, and the wood from the hills was no longer required by human industry—a great jungle began to prevail over all. The result was, that within twenty years, not only had the lake ceased to subside, but began once more to rise, and threaten the country with general inundation. This is only a single case out of many of precisely similar nature with which South America supplies us. We have had repeated occasion to allude to the diminution of rain in Oude, which the older inhabitants compare to the retiring of the tide, so manifest and gradual it is. In Switzerland it has been perfectly ascertained, that rivulets formerly full have shrunk or dried up coincidently with the denudation of the mountains, and that they have once more returned to their former size on the woods being restored. A beautiful spring, situated at the foot of a woody mountain in the island of Ascension, was observed to diminish in flow as the trees were cut down, and to vanish altogether when the wood disappeared. After a few years, during which no water flowed, the mountain became wooded again; when the stream once more began to flow, and, as the vegetation increased, returned to its former size. The destruction of wood, though at all times followed by a diminution in the flow of running water, is not invariably attended by a decrease in the fall of rain. Marmato, in the province of Popayan, is situate in the midst of enormous forests, and in the vicinage of valuable mines. The amount of the discharge of the streams,—here accurately measured by the work performed by the stamping machines which they drive,—was observed to decrease steadily as the wood was cut down; within the space of two years from the commencement of the clearing, the decrease of the flow of the water had occasioned alarm. The clearing was now suspended, and the diminution ceased. A rain-gauge was now established, when it appeared that the fall of rain had not diminished concomitantly with the flow of the streams. The apparent anomaly here presented does not affect the general doctrine, and is easily explained. The clearings were too local to affect the general condition of the climate; the rain which fell, however, instead of percolating, as was its wont through the soil, when shaded by trees, producing springs, rivulets, and brooks, now dried up, and was carried off in vapour as it fell. India, in nearly all these things, furnishes precise parallels to South America. A few years since a proprietor, in laying down some ground well watered by an excellent spring, for a coffee garden at Glenmore, in the Salem district, despite the advice of the natives, cleared the ground, when the supply of water vanished. At the village of Hoolbulley, near the head of the new Ghaut in Munzerabad, the jungle was cleared away, and in every case the diminution of water followed almost immediately,—in some cases the coffee-plants dying in consequence; the jungle was allowed to grow again when water returned, the springs were opened, and the rivulets and streams flowed afresh as formerly. Around Ahmednuggur, springs shaded by trees have invariably been observed to dry up almost immediately on the trees being removed. Having seen the result of the destruction of trees in diminishing the fall of rain, we come now to the converse state of matters, so as to establish the proposition by both varieties of proof. Unfortunately our evidences on this side of the question are much less numerous than those on the other, though equally uniform and pertinent, the propensity to remove or destroy being much more prevalent and active than to establish forests. The St Helena Almanac for 1848 gives particulars of the increase of the fall of rain within the last few years, attributable to the increase of wood: within the present century, the fall has nearly doubled. The plantations seem to have performed another service to the island. Formerly heavy floods, caused by sudden torrents of rain, were almost periodical, and frequently very destructive: for the last nine years they have been unknown. On the mountains of Ferro, one of the Canary Islands, there are trees each of which is constantly surrounded by a cloud: their power of drawing down moisture is well known to the people: the natives call them *garol*, the Spaniards *santo*, from their utility. The drops trickle down the stem in one unceasing stream, and are collected in reservoirs constructed for their reception. Thousands of similar instances might be quoted: Our own revenue surveyors, indeed, could supply an almost unlimited amount of information bearing on the same subject. The whole of this beautiful process depends on the simple laws of temperature, evaporation, and condensation. Trees shade the soil from the sun. They give off vapour during the day, and so mitigate heat, while they obstruct the direct rays from above,—they radiate out heat during the night, and occasion the precipitation of dew,—many plants being endowed with this faculty to such an extent as to collect water in large quantities from the air. The total quantity of dew believed to fall in England is supposed to amount to five inches annually—and the estimate appears to us to be a vast way under the truth: the average fall of rain is about twenty-five inches. Mr Glaisher states the amount of evaporation at Greenwich to have amounted to five feet annually for the past five years, and supposes three feet about the mean evaporation all over the world: On this assumption the quantity of actual moisture raised in the shape of vapour, from the surface of the sea alone, amounts to no less than 60,000 cubic miles annually; or nearly 164 miles a day. According to the observations of Mr Laidlay, the evaporation at Calcutta is about fifteen feet annually; that between the Cape and Calcutta averages, in October and November, nearly three quarters of an inch daily; betwixt 10° and 20° in the Bay of Bengal, it was found to exceed an inch daily. Supposing this to be double the average throughout the year, we shall, instead of three, have eighteen feet of evaporation annually ; or, were this state of matters to prevail all over the world, an amount of three hundred and sixty thousand cubic miles of water raised in vapour from the ocean alone !"—*Secretary's Report of the Proceedings of the Bombay Geographical Society for* 1849–50, p. 55.

Fossil Eggs of Snakes in the Freshwater Limestone at Beiber, near Offenbach. By Professor Blum.*

In the mineralogical cabinet of Herr C. Rossler, so preeminently rich in the natural curiosities of the Wetterau, I noticed several longish egg-shaped bodies, similar to others that I had previously seen at the house of Herr Witte, at Frankfort. And these were so much the more interesting, as some doubt still existed with regard to their origin ; some considering them to be inorganic concretions, whilst others regarded them as organic bodies—fossil eggs of snakes or lizards. But no one can doubt that the latter is the correct idea ; for the great uniformity, with respect to shape and size, of the different specimens, and I have seen more than fifty of them, would make them very remarkable as inorganic products, unless this view were supported by other proof. They are 8–10‴ long, and 5–6‴ thick. The ends taper off in so nearly a uniform manner, that one end scarcely appears broader than the other. They are altogether more cylindrical than the eggs of birds. Some specimens are here and there somewhat compressed ; which is easily accounted for by the soft condition of the shell in a recent state. Externally the surface is for the most part rough, like a wrinkled skin.

These bodies consist generally of calcspar ; a thinnish rind of which supports the outer surface, whilst the inside is more or less hollow and covered with little calcspar crystals. Sometimes an elongated calcspar, stretching from side to side,

---

* Leonhard and Bronn's Jahrb. fur Miner. 1849, p. 673–675.

occupies the egg. In one specimen some of the calcareous matrix, in which the eggs are found, has penetrated into the inside; in another it constitutes the whole substance of the fossil.

The forms exhibited by the calcspar crystals are a very acute elongated Scaleno-hedron, combined with the primary Rhombo-hedron ($R.^{12}$ R.), and a less acute Scaleno-hedron, with the acute Rhombo-hedron, f. (— $R.^2$—2 R.)

To enable me to make observations myself on the fossil eggs and the conditions under which they occur, HH. Rossler and Witte had the kindness to arrange an excursion to the place and shew me the exact spot.

These eggs are found in the Tegel-formation of the district of Offenbach, about half a league from thence, not far from the village of Beiber, in the quarries on the road towards Seligenstadt.

A brackish water-limestone, which is hereabout exposed in several quarries, exhibits a stratum of eight or ten inches thick, which is distinguishable by its soft loamy nature from the rock lying above and below it. In this the eggs are chiefly to be met with; for, according to the statement of the workmen, they occur very seldom in the hard strata. The whole rock belongs to the Paludina limestone, since Paludina acuta (*Littorinella acuta*, Al. Braun.) composes chiefly the mass. These little shells are here always enveloped in a calcareous coating, and are held together by a more or less pure calcareous matrix, so that the rock has a more or less oolitic appearance. At isolated spots calcspar traverses the rock in lines, or disposes itself in drusy cavities, in columnar spars, or in crystals. The limestone is partly white, partly more or less coloured yellow by hydrated oxide of iron. Sometimes the colour is arranged in stripes, which traverse the eggs also, when the latter are filled with the matrix.

The stratum that contains the eggs is moist, and so soft that large pieces can with difficulty be removed; by exposure to the air it becomes somewhat harder and gradually becomes white and chalky. The eggs occur in this bed either singly or in groups. There are found also both in this softer, as well as in the harder part of the rock, a large *Helix*, perhaps H. *Mattiaca*, Steininger, and more plentifully a small *Helix*; also Clansilia bulimoides and Dreissenia Brardi.

From what we have said of the characters and contents of these fossils and of the conditions under which they exist, it results that all idea of their inorganic concretionary origin must fall to the ground. Concretionary bodies are formed from within outwards, but here exactly the opposite has taken place; lime in solution has permeated the parchment-like shell of the egg, and has been gradually deposited on its inside, and thus preserved the form of the egg after the organic substance itself had disappeared.

I consider, therefore, these fossils to be the eggs of snakes, perhaps of a coluber; they are, however, somewhat too large for eggs of the colubers or lizards now existing in the neighbourhood.

Lizards lay their eggs in warm sand, but many snakes lay them in moist ground or mud, even under water. Such animals could have lived here, on the banks of the Maine and the Rhine, and have deposited their eggs in the calcareous mud, where, perhaps, an increase of calcareous matter not only prevented the hatching, but furthered the petrifaction of the eggs.—*Quarterly Journal of the Geological Society*, No. xxii. p. 42.

Sixteenth Letter on Glaciers,—addressed to Professor Jameson. (1.) *Observations on the Movement of the Mer de Glace down to* 1850. (2.) *Observations by Balmat, in continuation of those detailed in the Fourteenth Letter.* (3.) *On the gradual passage of Ice into the Fluid State.* (4.) *Notice of an undescribed Pass of the Alps.* By Professor J. D. FORBES. Communicated by the Author.

MY DEAR SIR,—Having had the good fortune once more to spend a few (though very few) days amongst the glaciers of Chamouni last summer, I avail myself of your kind permission to carry forward the account of my observations, which has now, for a period of eight years, been regularly communicated to the readers of your Journal. As my stay was limited by imperative engagements to little more than a week, I was prevented from undertaking a continuous series of observations on the movement of the ice. I was fortunate, however, in obtaining materials for the correction and extension of certain parts of my Map of the *Mer de Glace*, which were deficient in my former observations, especially as to the exact form of the basin of the great *Glacier du Géant*, which I had only visited once before, on occasion of the passage of the Col of that name in 1842. This year I traversed again all the difficult part of that glacier, and took angles with the theodolite from the upper part of the basin, immediately under the *Aiguille du Géant.* But as these observations can have little interest until reduced into the form of a corrected edition of the Map, I shall say nothing of them here.

It will be recollected by some of your readers that a remarkable stone called "La pierre platte,"* was one of the earliest points whose position was ascertained by me in 1842. Its daily motion was watched by me during that summer,† and its annual motion was ascertained by renewed observations in 1843, 1844, 1846, and again this year. I measured the distance along the ice from the original position of the "Pierre platte" on the 27th June 1842 (ascertained by reference to fixed marks on the rocks) to its position on the 12th July 1850, and found it to be 2520 feet. But, of this distance, 1212 feet had been travelled at my previous observation on the 21st July 1846, leaving 1308 feet during the last four years against 1212 in the first four. When more accurately stated and compared, the mean annual and daily motions will stand as follows:—

	1842–3.	1843–4.	1844–6.	1846–50.
Daily motion, in INCHES,	9·47	8·56	10·65	10·81
Annual motion, in FEET,	288·3	260·4	323·8	328·8

We cannot infer, with absolute certainty, that the slight in-

* Lying on the surface of the *Glacier de Léchaud* (in the upper part of the *Mer de Glace*), and carried along by the motion of the ice. It is marked C in my Map of the Glaciers.

† Travels in the Alps of Savoy, 2d edition, p. 139–40.

crease of velocity here noticed since 1844 is due to a change in the conditions of the glacier (although I believe that the recurrence of several snowy seasons and the very marked increase of the volume and extent of the glacier during these years would produce such an effect), because it has moved nearly half-a-mile from its position when first observed, and the part of the glacier on which it now lies may be subject to different accelerating and retarding causes.

It is mentioned in my Thirteenth Letter, page 4, that I marked a fine solitary block towards the centre of the Mer de Glace opposite "Les Ponts" with the letter V in 1846, and that I took angles for fixing its place with reference to the adjacent rocks. It was then about 760 feet distant from the west bank I had little difficulty in recognizing the block in 1850, although it had travelled a great distance and was considerably lower than the Montauvert. It had preserved its parallelism to the shore, for I found it at almost the same distance from the west bank as at first; and by measuring carefully along the side of the glacier, I estimated its progress in four years, from 30th July 1846 to 13th July 1850, at 3255 feet. This gives, for the mean motion in 365 days, 822·8 feet, or the mean daily motion 27·05 inches, which is remarkably large. Its position is very near the point of one of the "dirt-bands," but a little nearer the western bank. It lies, however, *on* the band.

I shall now give the sequel of my guide Auguste Balmat's observations on the motion of the Glacier des Bois (the outlet of the Mer de Glace), and of the Glacier des Bossons, since the period to which the table in my Fourteenth Letter extends, which will be found to embrace *continuous* observations, by periods of a few weeks from the 2d October 1844 to the 21st November 1845. They were continued in like manner until the 19th February 1846, when they were interrupted by Balmat's illness, which was accompanied by inflammation of the eyes. But in October of the same year they were resumed, and were continued without intermission until the end of June 1848, embracing altogether a period of nearly four years, with only eight months' intermission. It is necessary to observe that the station on the glacier of Bossons was altogether changed after the above mentioned interruption, being transferred from the west to the east side (in the same region of the glacier), and it was 340 feet from the bank. The station on the Glacier des Bois was almost unchanged, and was about 280 feet from the north bank, between the Côte du Piget and the acclivity of the Chapeau. I have added a column giving the mean of the temperatures of the several periods of observation, carefully calculated from the published observations at Geneva and the great St Bernard, on the same principle as I have fully explained in my Fourteenth Letter above referred to. The comparisons of the temperature and the rate of motion lead to conclusions similar to those which I have drawn in that paper from the earlier observations, the general observation always holding that the acceleration in spring is in a greater proportion to the temperature than at any other season of the year, on account of the great influence of the melting snows in imparting fluidity to the glacier masses. I do not mean that the comparison leads always to consistent results. I do not think that the causes of the comparative acceleration of one glacier and retardation of another have yet been clearly brought out, though I conceive that accurate local observations, combined with such measurements, would gradually but surely unveil them. Nor do I mean to affirm that measurements made with so much labour and trouble, and under circumstances even of personal danger at certain seasons of the year, are irreproachable in point of accuracy. I think it even probable that oversights have occurred; but I have very strong reason for confiding in the absolute fidelity with which the observations have been made and transmitted to me. Circumstances have transpired since my last publication which increase this confidence; and I should be ungrateful if I did not once more publicly acknowledge, whilst giving to the world the sequel of observations made under such circumstances that their resumption is scarcely probable, the lasting obligations which I owe to the zeal, fidelity, and disinterestedness of my worthy though humble friend and guide.

TABLE shewing the mean daily motion in inches of the Glaciers of Chamouni deduced from Balmat's Observations, and continued from the Fourteenth Letter.

Intervals of Observation.	Mean Daily Motion in Eng. inches.				Temp. Centigrade of Air.*	Remarks.
	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.		
			west side		°	
1845. Nov. 16 to Dec. 16	14·0	10·9	30·2	6·4	−1·47	
Dec. 16 to Jan. 19	12·0	5·7	18·8	10·0	−4·19	
1846. Jan. 19 to Feb. 19	16·1	5·1	16·9	13·0	−0·16	
(Observations interrupted by Balmat's illness.)						
			east side.			
Oct. 12 to Nov. 19	21·8		10·8		1·65	16th Oct. Snow at Montanvert.
Nov. 19 to Dec. 20	24·0		13·1		−4·41	
Dec. 20 to Jan. 18	24·5		12·8		−5·88	
1847. Jan. 18 to Mar. 4	31·5		14·5		−4·82	Vast quantity of snow. Destructive avalanches.
Mar. 4 to Apr. 12	34·5		13·9		−1·08	
Apr. 12 to May 14	37·3		19·7		3·10	
May 14 to July 2	34·2		22·6		9·97	Snow disappeared on Bossons, 2d. week of May: on Bois, 3d week of May.
July 2 to July 23	30·5		23·1		13·88	
July 23 to Aug. 16	34·0		25·8		11·89	
Aug. 16 to Sept. 9	44·7		23·5		9 65	
Sept. 9 to Sept. 28	37·7		22·6		7·95	
Sept. 28 to Oct. 18	32·2		21·5		5·34	
Oct. 18 to Nov. 6	30·7		14·5		3·41	
Nov. 6 to Nov. 27	30·2		10·7		0·24	
Nov. 27 to Jan. 10	24·4		10·5		−3·74	
1848. Jan. 10 to Feb. 19	26·5		14·5		−5·79	
Feb. 19 to Apr. 1	23·5		12·6		−0·64	
Apr. 1 to May 3	33·8		18·8		4·93	
May 3 to June 6	35·3		17·6		8·68	
June 6 to June 30	43·8		17·6		11·57	

In my former Letters I have taken occasion to mention experiments and observations which have occurred from time to time of a nature to confirm the fundamental hypothesis of the *quasi* fluidity of the ice of glaciers on the great scale, and I cannot doubt that these incidental remarks have tended to diminish the natural incredulity with which that theory was at first received in some quarters. I have now to cite a fact of the same kind established by a French experimenter, M. Person, who appears not to have had even remotely in his mind the theory of glaciers when he announced the following fact, viz.:—That ice does not pass *abruptly* from the solid to the fluid state: That it begins to

* Mean of Geneva and Great St Bernard.

soften at a temperature of 2° centigrade below its thawing point: that, consequently between 28·°4 and 32° of Fahrenheit, ice is actually passing through various degrees of plasticity, within narrower limits, but in the same manner that wax, for example, softens before it melts. M. Person deduces this from the examination of the heat requisite to liquify ice at different temperatures. The following sentences contain his conclusions in his own words:—"Il parait d'après mes experiences que le ramollissement qui précède la fusion, est circonscrit dans une intervalle d'environ 2 degrés. La glace est donc un des corps dont la fusion est la plus nette; mais cépendant le passage de l'état solide à l'état liquide s'y fait encore par degrés, et non par un saut brusque."*

Now it appears very clearly from M. Agassiz' thermometrical experiments, and from my own observations, that from 28° to 32° Fahr. is the habitual temperature of the great mass of a glacier; that the most rigorous nights propagate an intense cold to but a very small depth; and I am perfectly convinced that in the middle and lower regions of glaciers which are habitually saturated with water in summer, the interior is little, if at all, reduced below the freezing point, even by the prolonged cold of winter; it would be contrary to all just theories of the propagation of heat if it were otherwise, when we recollect the enormous mass of snow which such glaciers bear during the coldest months of the year, is a covering sufficient to prevent any profound congelation in common earth; and admitting that ice is probably a better conductor of heat than the ground, it is quite incredible that a thickness of many hundred feet of ice, saturated with fluid water, should be reduced much below the freezing point, or should even be frozen throughout. And that it is not, the striking testimony of the continued stream of water issuing all winter from under the ice can hardly fail to convince us; still more, the circumstance mentioned in my Fourteenth Letter, that even in the month of February the source of the Arveron becomes *whitish and dirty*; *as in summer*, *before a change of weather*, proving (as I have there remarked) that "in the middle of winter a temporary rise of temperature over the higher glacier regions (which is the precursor of bad weather) not only produces a

* Comptes Rendus, 29th April 1850.

thaw there, but finds the usual channels still open for transmitting the accumulated snow-water."

It thus appears quite certain that ice, under the circumstances in which we find it in the great bulk of glaciers, is in a state more or less *softened* even in winter; and that, during nearly the whole summer, whilst surrounded by air above 32°, and itself at that temperature, it has acquired a still greater degree of plasticity, due to the latent heat which it has then absorbed.

I have mentioned that the observations of this and some previous summers have enabled me to extend the survey of the valley of Chamouni beyond the limits to which my Map was originally confined. I have also obtained a great number of approximate altitudes of all the highest summits of the chain of Mont Blanc, from the extended base which the distance from the Mont Breven to the Croix de Flégère (above 15,400 feet) has afforded me. But the results are as yet only partially calculated. I have also made some additions to our knowledge of the geography of the eastern part of the chain of Mont Blanc, by examining the Glacier of La Tour in its whole extent, which proved the configuration of the mountains to be different from what has been represented on all the maps and models which I have seen. The Glaciers of Argentière and La Tour are separated throughout by a rocky ridge, but the Glaciers of La Tour and Trient all but unite at their highest parts, and the main chain is prolonged with scarcely a break in the north-east direction, sending off only a spur towards the Col de Balme, which, perhaps from being the political boundary of Savoy and Switzerland, has been represented generally on an exaggerated scale. What surprised me most, was the great elevation of the axis of the chain at the head of the Glaciers of La Tour and Trient. I found it barometrically to be 4044 feet above the châlet of the Col de Balme, which, from five comparisons made with the observatory at Geneva, is 7291 English feet, or 2220 mètres above the sea, a result agreeing closely with the recent measurement by M. Favre, which is 2222 mètres. Adding this result to the former, we obtain 11,335 English feet for the height of the granitic axis at the lowest point between the Glaciers ofLa Tour and Salena on the side of the Swiss Val

Ferret. By a single direct barometrical comparison with Geneva, I obtained 11,284 English feet above the sea, or 140 feet higher than the Col du Géant. I was successful in traversing the Glacier of Salena to Orsières the same day, a pass which has not before been described, and which has this interest, in addition to the singular wildness of the scenery, that it includes those regions of beautiful crystallized protogine, here *in situ*, which have been known to geologists hitherto chiefly from the numerous moraines which they form in the valleys of Ferret and of the Rhone, and especially the majority of the blocks of Monthey, which have been derived, according to M. de Buch, entirely from this region of the Alps. I remain, my dear Sir, yours very truly,

JAMES D. FORBES.

To Professor JAMESON.

Astronomical Notices. By C. PIAZZI SMYTH, Esq., F.R.S.E., Professor of Astronomy in the University of Edinburgh. Communicated by the Author.*

These notices are merely, in general, the contents of such letters as are continually arriving at the Royal Observatory on the Calton Hill, from various members of the large body of correspondents, at home and abroad, which the establishment of such an Institution necessarily gives rise to.

Many communications of much importance are thus received, and it will doubtless be a matter of interest to some members of this Society, as well as of advantage to the authors, to lay the papers on the table for inspection during a few days, merely saying a few words in explanation at the present time.

Some care in selection certainly is necessary; for when one gentleman writes, as he did lately, from the north of Scotland, "that he considers the distance of the earth from the sun is not known, and that it is not understood what keeps the sea within its bounds; that he will be glad of my opinion thereon, as he intends to bring those matters to a final and satisfactory conclusion;" and when another writes "that a French astronomer has concluded, from his own observations on the moon, that in seven years that body will be in contact with the earth;" or another, "that he can predict all the changes of the weather by the configurations of the satellites of Jupiter;" when men will write such things, it is sufficient expenditure

* Read before the Royal Society, Dec. 16, 1850.

of time to have written answers to them, as civil as possible, without wasting more by repetition of them to the Society: and, fortunately, such correspondents form but a small part of the whole.

Having mentioned the weather, I may allude, before coming to the astronomical matter, to the diagram on the table drawn by the Society's anemometer, now at the Observatory. The storm of last Saturday forms an interesting feature; the strength of the wind was confined to within from 2 h. to 4 h. P.M., and reached in the momentary gusts a force of 23 lb. on the square foot; but as more accurately measured by a revolving anemometer, the speed was equal to 39 miles per hour; between 3 h. 30 m. P.M. and 4 h. 10 m. P.M., a change in the direction occurred from S. 20 E. to S. 70 W.; seeming to shew, on the hurricane theory, that the centre of the whole at its nearest approach was N. 60 W., and the direction of progress about N. 30 E. But to fix this decidedly, observations from other places are necessary.

The first astronomical matter to which I will call attention, is a work recently written by the author, Professor Loomis, "on the recent progress of Astronomy, especially in the United States," published in the present year at New York.

The highest astronomical authority in this country recently declared that the Americans, though last in the field of Astronomy, were already able to give lessons to their old masters; and this work eminently shews their rapid progress.

Chapter I. contains the advances in the knowledge of the Planetary System in the last ten years; Chapter II., of the Cometary System; and Chapter III., of the Fixed Stars and Nebulæ; while the IVth contains accounts of the United States' Observatories, now 15 in number; of the astronomical expedition to Chili; of the astronomical results of public surveys; of the determination of longitude by the electric telegraph; of astronomical publications; and the manufacture of telescopes in the United States.

In every department, both practical and physical, the most satisfactory indications are shewn; and if they go on as they have been doing, and we do not do more than we have been doing, we cannot but be left behind before long.

The second matter I have to bring forward, is Dr Locke's account of his electric observing clock, which has been used with such signal advantage in improving the accuracy of astronomical observations; and by whose aid the differences of longitude between all the principal American cities are known more exactly than that between Greenwich and Paris. Small wonder then, that when the absolute longitude from Europe should labour under so great an uncertainty as it does, that the Americans should now think of setting up a new meridian for themselves.

On the invention being published, and favourably reported on, Congress gave Dr Locke the liberal sum of 10,000 dollars for the purpose of enabling him to establish one of his electric clocks in the National Observatory of Washington, and also retain a handsome reward for himself. The pamphlet in question is a very full account of the excellent manner in which he has performed his duty.

The papers have been ringing, during the last few days, with accounts of the discovery of a third ring to Saturn, by Professor Bond of Cambridge, Massachusetts; but the account of what has been seen is so very indistinct, that some have asserted that it is only the same division of the external ring seen at various times by Captain Kater, Encke, Quetelet, and Messrs Dawes and Lassel. There seems more probability, however, of its being a division, or rather a series of divisions, in the inside of the inner ring, which have been seen. This, together with the observations of the former astronomers, confirms remarkably Laplace's theoretical view of the constitution of the Saturnian ring.

Messrs Bond's and Lassel's joint discovery of the eighth satellite of Saturn, Hyperion, is much more satisfactory, and the new member stands thus amongst the old:—

	Revolution in days.
Mimas, . . .	0·94
Enceladus, . . .	1·37
Tethys, . . .	1·89
Dione, ...	2·74
Rhea, ...	4·52
Titan, ...	15·95
Hyperion, . . .	21·18
Japetus, . . .	79·33

The total solar eclipse of July 28th, 1851, presents a strong inducement to travel at that season of the year. Eclipses are seen total over so small a portion of the earth, that, were a person to remain fixed in any one spot for 500, or even 1000 years, he might not see one; while, if he travel about the world, he might see one almost every year. Sometimes, indeed, he would be obliged to visit the North Pole and sometimes the South, or other equally inclement and impassable regions; but the time of total obscuration for next year, traversing the north-east of Europe, is easily accessible. Many persons present, doubtless, witnessed the annular eclipse of a few years ago here, and thought much of it: But however interesting and striking it may have been, it is not to be compared with a total one; for the sun's surface is so excessively bright that, if the smallest particle of its disc be left uneclipsed, there is still a general daylight; but in an actual total eclipse, there is nocturnal darkness, and a host of interesting phenomena relative to the physics of the sun and moon come out, which can never be inquired into at any other time.

Professor Littrow of the Vienna Observatory, has sent a map of the path of total obscuration, the central line of which passes through the points whose latitudes are lat. N. 60°, 55°, and 50°, and long. E. of Greenwich, 6° 58′, 17° 48′ and 27° 38′. The breadth of the line of observation is equal to about 130 geographical miles. With these data, any person may draw the lines on his own map, and securely choose his place for observing the phenomena. Arrangements beforehand in distributing watchers all along the line, rather than grouping them at one place, would evidently be desirable, to avoid local clouds, &c.

The two new planets, Victoria and Egeria, discovered, the first in September by Mr Hind, the other in November by M. Gasparis of the Neapolitan Observatory, stand thus amongst the rest of the planets:—

		Distance from Sun.	Revolutions in Solar days.
	Earth,	1·00	365·26
	Mars,	1·52	686·98
The Asteroidal Group.	Flora,	2·20	1193·25
	Egeria,	.	
	Victoria,	2·33	1303·50
	Vesta,	2·36	1325·15
	Iris,	2·38	1341·64
	Metis,	2·39	1345·85
	Parthenope,	2·42	1379·39
	Hebe,	2·43	1379·99
	Astrea,	2·58	1511·10
	Juno,	2·67	1682·12
	Ceres,	2·77	1686·51
	Pallas,	2·77	1686·51
	Hygeia,	3·27	2160·00
	Jupiter,	5·20	4332·58

The first orbit of Victoria which reached me was that computed by Mr Hugh Breen, assistant-astronomer at the Greenwich Observatory. The close approach to accurate determination made already in the elements of this new planet, may be gathered from the following values of the mean distance as determined by Mr Breen and other competitors:—

Hugh Breen,	2·336
Yvon Villarceau,	2·335
Rumker,	2·336
Fearnly,	2·330

Ephemerides of Victoria and Egeria, as well as of Faye's comet, have been sent me by the Rev. R. Sheepshanks, editor of the Notices of the Royal Astronomical Society; and by Lieutenant Stratford, R.N., Superintendent of the Nautical Almanac; and observations of all three objects are in great request.

The following places may be of service to intending observers.

December 20, 1850.

	Right Ascension.			Declination.		
	h.	m.	s.	°	′	″
Victoria,	0	4	5	+ 5	8	8
Egeria,	1	33	6	+10	53	16
Faye's comet,	22	6	6	− 5	58	18

December 30, 1850.

	h.	m.	s.	°	′	″
Victoria,	0	17	9	+ 5	47	13
Egeria,	1	35	14	+11	59	0
Faye's comet,	22	26	0	− 4	58	36

Faye's comet must be an object of especial interest to every astronomer, on this, its first predicted return. Hundreds of comets have had orbits computed, but from a variety of causes, only three have hitherto been observed to return; Faye's comet makes the fourth; its period is about seven and a half years.

It is extremely faint, and requires a very powerful telescope to be perceived. Such have not hitherto been common in Scotland, but Mr Grant of Elgin is procuring an object glass of 11·5 inches aperture, from Andrew Ross, optician, London; while the stand is being executed by Messrs Ransome and May of Ipswich, the makers also of the new Meridian instrument of Greenwich.

This speaks sufficiently for the accuracy of the workmanship; but in equatorial mountings, to prevent small vibrations, the further element of solidity is necessary, and it will be gained in an unprecedented degree in Mr Grant's instrument, for it will chiefly consist of cast-iron, and will weigh over 7 tons; one piece of it alone, being about 4 tons.

Were it not for private enthusiasm, England would be left quite behind in this branch of astronomy; for while the Russians, Germans, and Americans, are continually ordering for their Observatories the largest telescopes that can be made, the English Government will not supply any such to the British ones.

Private men here have raised the reflecting telescope to a height of perfection utterly beyond that attained elsewhere, but a villainous climate prevents them from using it. Seeing this, the British Association and the Royal Society of London recommended to Government to take up the subject, and send one of these specimens of British skill to the clearer climate of Australia; but it has just transpired, that the Government refuses to recommend the measure on account of the—expense!

There is, however, a more favourable part of the world still than Australia, and fortunately British ground, viz., the high table-land of India, where a Rosseian reflector, raised high into the purer regions of the atmosphere, and with the planets passing its zenith, would inevitably distance all that foreigners can do with their present instruments and in their existing observatories, and would also mark the present epoch as one of signal discovery in the annals of astronomy.

With this idea, my friend W. S. Jacob, Esq., H.E.I.C. Astronomer at Madras, finding the instruments of that Observatory small and worn out, and requiring renewal, proposed to the Company to send out rather a large reflector, and let him establish it on the Nilgherry hills, at a height of 6000 feet above the sea. But this request, which must have resulted in so much scientific credit for the Company, was refused on the score of—expense. Then he offered to make the telescope there at a cost of not more than the small sum (insignificant when the result is considered) of £500, but this was refused.

He has, therefore, now begun making a 20-feet reflector at his own private expense, and all he asks of the Company is to let him establish it, with a part of the assistants of the Madras Observatory, on the above-mentioned hills; and if this be refused, he is so strongly impressed with the rich results that must inevitably follow the placing of one of the modern British reflectors on so favourable a geographical position of the Nilgherry hills, that he is determined, in such a case, to throw up his appointment, and establish the telescope on the heights himself, looking to support himself and family meanwhile by coffeé or indigo planting, or anything else which the climate and soil of the country may allow.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *New Theory of Polar Lights.*—Mairan, and, more lately, Dalton, have explained this phase of the aurora by a hypothesis of polar beams, long fiery rods of solar atmosphere, according to the one, of red-hot ferruginous particles, according to the other, seen in perspective, as they lie in the direction of the magnetic force. A little acquaintance with the phenomenon—the rushing and tilting of the beams against each other, one beam occasionally rising from the horizon, passing through the centre of the crown and beyond it—would shew the improbability of this hypothesis. I am persuaded, that the phenomenon of the corona borealis is produced in a narrow horizontal stratum of the earth's atmosphere. Thanks to the discoveries of Dr Faraday, we do not require a ferruginous sea, in order to have polarized particles; the watery crystals that inhabit the upper regions of the atmosphere can themselves assume a polar state, determined by the passage of electric currents; and we have only to complete this fact by a hypothesis of luminous electric discharges seen refracted by these crystals, the position of visibility of the refracted rays depending on the angles of the crystals, and the deflections from the direction of the magnetic force which they suffer, by the electric currents. Such an hypothesis, which occurs at once when an optical phenomenon has to be accounted for, would explain these remarkable auroral clouds, so often seen in connection with the aurora itself; it would also serve to explain the appearance of the arch at certain altitudes, lower for lower altitudes, determined by the position of the source of light, direction of the magnetic force at the place, and the

effect of the electric current in deflecting the crystals. The crystals successively deflected by electric currents would also exhibit the rushing pencils or beams.

It need scarcely be remarked, that differently-formed crystals might give rise to different phases of the phenomenon; while reflection might be combined with refraction in certain cases, especially in the case of arches seen south of the anti-dip. Such an hypothesis evidently assumes a source of light, independent of these optical resultants, and the pulsations seen in many auroræ may be real luminosities.

It is hazardous, in the present ill-arranged state of auroral observation, to offer so rude a sketch of a new hypothesis, although we may suffer a considerable defeat in very good company.—*J. A. Broun, Esq.—Proceedings of the Royal Society of Edinburgh*, vol. ii., No. 39, 1850, p. 349.

2. *J. A. Broun, Esq., on the probable cause of the Diurnal Variation of Magnetic Variation.*—It has been customary, however, to give theories of the cause of magnetical variations, with reference solely to the diurnal variations of the magnetic declination (and not unfrequently with a very indifferent knowledge of the facts with respect even to that element.) I venture to say, that it will only be from a careful comparison of the whole facts relating to the motions of a freely suspended dipping-needle, not for one place, but for different and distant portions of the earth's surface, that a satisfactory theory will be obtained. The attempt to deduce one from a consideration of the declination variations alone, can only be likened to a similar attempt with reference to planetary motions, the apparent position of the planet being studied without any relation to the direction or rate of motion of the place of observation.

Dr Lloyd, who has done so much for magnetical science, has lately brought forward a discussion of his declination observations, which he considers strongly in favour of the theory that the diurnal variations of magnetic declination are due to the sun's heating effect upon the earth, in opposition to the atmosphere. I venture also to offer my guess, founded upon a consideration of various meteorological facts, that it is in the atmosphere, and not the earth, that we shall find seated the secondary causes of magnetic variations. In the meantime, it is *facts* that are wanted.—*J. A. Broun, Esq.—Proceedings of the Royal Society of Edinburgh*, vol. ii., No. 39, p. 342.

3. *Dr Faraday on the Diurnal Movements of the Magnetic Needle.*—The last Bakerian Lecture was delivered by Dr Faraday, illustrated by experiments. After alluding to the experiments of Padre Bancalari, the lecturer shewed the opposite magnetic condition of oxygen and nitrogen; the former, when enclosed in a bubble of glass, is always attracted by the magnet, the latter repelled. In common with iron and some other metals, oxygen loses magnetism on the application of heat, and regains it again on becoming cold. In this fact Dr Faraday finds the cause of the diurnal movements of

the magnetic needles all over the world, as exhibited at the respective Observatories; and he explains the apparent anomalies which occur at St Helena and Singapore on the hypothesis induced from the whole of the phenomena. The lecturer, in closing, stated that the explanation was to be received as conjectural, although, at present, as sufficiently satisfying the theory.—*Athenæum*, No. 1206, p. 1282.

4. *On a Cloud of Dust which obscured the Sun for two days in Russia, on the* 29*th and* 30*th April* 1840, *during a clear sky and quiet weather.*—This powder was furnished Ehrenberg by M. Eichwald. Microscopic examination brought to light forty-nine animal forms, soft portions of plants, a few crystals, a morpholite, and some sand. This powder is distinguished from that of the trade winds by some prominent forms. Ehrenberg believes that there is reason for concluding that this meteoric powder is neither a terrestrial powder nor simple volcanic cinders.

5. *Shower of Aërolites.*—Extract of a letter from Mr Richardson, dated Jerbah, 25th January 1850:—

"I will trouble you by the mention of the astronomic phenomenon which terrified or arrested the attention of the inhabitants of the whole of this coast some two months ago. This was the fall of a shower of aërolites, with a brilliant stream of light accompanying them, and which extended from Tunis to Tripoli, some of the stones falling in the latter city.

"The alarm was very great in Tunis, and several Jews and Moors instinctively fled to the British Consulate, as the common refuge from every kind of evil and danger.

"The fall of these aërolites was followed by the severest or coldest winter which the inhabitants of Tunis and Tripoli have experienced for many years."

6. *Destruction of Forests in Madeira.*—The progressive destruction of the forests, since the first discovery of the island of Madeira, has very materially modified the climate, by making it less humid. The smooth surfaces of leaves of the trees of the laurel tribe, cool rapidly by radiation, when the sky is clear; and the dew being consequently. deposited profusely upon them, they collect and distil, as it were, water in great abundance from the atmosphere. When the island was first discovered, and for many years afterwards, while the northern mountains were covered with evergreen trees, the river Socorridos—the most considerable in Madeira—which runs through the Curral, was found to be sufficiently deep to float timber to the sea, which it enters near Camea dos Lobos. It is now reduced, when not suddenly flooded, to a small stream, almost lost in the loose rocks which occupy its channel. It would appear that the attention of the settlers was called, at a very early period, to the injurious effects produced by the rapid diminution of the forests in a country; where, from the dry and porous character of the soil, and the warmth of the climate, moisture becomes the great principle of

fertility. A law was made, and is still in existence—though, unhappily, like other laws in this island, very rarely or very imperfectly enforced—which made it penal to cut down a vinhatico or til, if found near a fountain or on the banks of a river. The same effects have been found to follow, in a greater or less degree, in all countries, whether tropical or not, from the diminution of timber, whether produced by the extension of cultivation or by other causes.—*Dr Mason and Mr Driver on Madeira*, p. 234.

7. *Supposed Change of Climate in New Zealand.*—Some of the gold of California is beginning to find its way here, in exchange for flour and timber, &c. Ready-made wooden houses, framed and fitted, have been exported in considerable quantity to California, and sell extremely well. Summer good,—crops abundant. It is the opinion of nearly every one, that the climate has undergone a great change since the occurrence of the violent earthquake, which we experienced about eighteen months since, and a change for the better. It is difficult at first to perceive any connection between earthquakes and the weather; but if electricity be supposed to be a principal agent upon which earthquakes depend, the connection will become more apparent. Be this, however, as it may, the fact is certain, that since our earthquake the weather has been more genial than it used to be. Another circumstance, however, which is a common matter of observation, is this,—that as the weather has become more favourable to vegetation, it has been less so to health; for colds, influenzas, and slight feverish attacks, have been very prevalent of late, and much more frequent than what they formerly were. During the first four or five months after I came here, it was exceedingly rare to hear any one cough; but this last summer nothing has been more common. How fearful the cholera appears to have been in England. No case, I believe, has been met with south of the line; the same thing is asserted of hydrophobia, but I do not know with what truth.—*Extract of a Letter from Dr D. Monro, New Zealand.*

GEOLOGY.

8. *On the Results of the latest Researches explanatory of Carbonic Acid Exhalations.* By G. Bischoff.—Bischoff found that carbonic acid was gradually separated from carbonate of lime, by silicic acid with the co-operation of boiling water. This decomposition took place, whether the silicic acid was in a soluble or insoluble condition; for even finely pulverized quartz decomposed the carbonate of lime, the process, however, in that case being rather slower. Carbonated oxydule of iron (Spatheisenstein) and the carbonate of magnesia behave in like manner; the latter is decomposed even more easily and in greater quantity than the carbonate of lime. The more facile decomposition of carbonate of magnesia is shewn by the fact that even boiling water by itself separates the carbonic acid from it, this not being the case with the carbonated oxydule of iron. When, therefore, either limestone, dolomite, or sparry iron occurs at a depth

beneath the earth's surface where boiling water heat exists, and water has access, carbonic acid will be driven off from these carbonated salts.

The Soffoni, on Monte Cerboli, &c. in Tuscany, discharging boiling-hot vapours from crevices in the limestone, must come from a depth where boiling heat exists, and it is very probable that the accompanying carbonic acid arises from the above-mentioned causes. The same must be admitted for the carbonic acid discharged so abundantly in the neighbourhood of the Laacher-See, in the volcanic Eifel, and in other places. These exhalations proceed from the clay-slate formation. According to the laws of the increase of temperature towards the centre of our earth, we may calculate that boiling heat exists at a depth of about 8600 feet in these districts, and this depth is certainly within the limits of the clay-slate formation, which is calculated to be at least a mile (German) thick. Calcareous beds (Transition limestone) and quartzose rocks occur at this depth, waters penetrate thereto, and carbonic acid is separated from the limestone as in the above-mentioned experiments. To account therefore for the origin of carbonic acid exhalations, we need no more assume that the focus must be where red heat exists, which presupposes a depth of at least five miles (German); for the clay-slate, or any other sedimentary formation, may be the seat of the evolution of the gas, since only in the moderate depth of about half-a-mile (German) the materials required are present. T. R. J.—*The Quarterly Journal of the Geological Society*, No. 22, vol. vi., p. 40.

ZOOLOGY.

9. *On the Increase of the Nail and the Hair in Man.* By M. Bertholdt (L'Institut., No. 846, 93.)—The growth of the nails in children is more rapid than in adults, and slowest in the aged. It goes on more promptly in summer than in winter, so that the same nail which is renewed in 132 days in winter, requires only 116 in summer,—a fact depending on the *vis vitalis*, which seems to be proportional to it. The increase in the nails of the right hand is quicker than for the left; moreover, it differs for the different fingers, consequently, most rapidly for the middle finger, with nearly equal rapidity for the two either side of this, slower in the little finger, and slowest in the thumb. For the middle finger of the right hand, the nail grew 12 millimeters in 106 days; for the small finger of the left hand required 88 days more than for those of the right, and also there were produced in this time 3 millimeters less than on the right hand.

The growth of the hair is well known to be much accelerated by frequent cutting. It forms more rapidly in the day than at night, and in the hot season than in the cold. But it is difficult to determine the precise rates.

It results from the tables accompanying the memoir of M. Bertholdt, that the growth of the hair and nails, as well as that of the epidermis, pertains to the secretions, and not to the organic structure proper. For (1.) the quantity of each formed, corresponds very nearly with that of the peripheric secretions, especially with transpiration, it increasing in summer, whilst, on the contrary, the growth and nutrition of the body are most rapid in winter, so that the weight of a man, as was observed by Sanctorius, Liennig, and Reil, is greatest in winter; (2.) the growth of the hair being least during the night accords with the diminution of all the secretions, as that of transpiration, the formation of carbonic acid, the urinary, lacteal, and bilious secretions.

10. *Protection against Musquitoes.*—On landing, I walked towards the rapids, about a quarter of a mile up the stream. The flies and musquitoes made their appearance as soon as I entered the woods, and jumping down into the bed of the stream with the intention of sketching the mass of water that was foaming down over the rocks, I was instantly surrounded by such swarms that there was no getting on without a smudge. Even standing in the midst of the smoke, so many still clung to me, that my paper was sprinkled with the dead bodies of those killed, as I involuntarily brushed my hand across my face. We took refuge on the sand, at a distance from the woods, and here were comparatively free from them. But here their place was supplied by sand flies, the *brûlots* or "nosee-ems," an insect so minute as to be hardly noticeable, but yet more annoying where they are found than the black flies or musquitoes, for their minuteness renders musquito-nets of no avail, and they bite all night in warm weather, whereas the black fly disappears at dark. Such is their eagerness in biting, that they tilt their bodies up vertically and seem to bury their heads in the flesh. We found, however, that an ointment of camphorated oil was a complete protection, making a coat too thick for them to penetrate, and entangling their tiny wings and limbs.—*Agassiz on Lake Superior*, p. 55.

11. *American Zoological Journal.*—We take pleasure in announcing the speedy appearance of a Zoological Journal at Cambridge (Massachusetts), under the direction and editorship of Professor Agassiz. Its memoirs will bear upon whatever pertains to animal life, its development, anatomical structure, physiological relations, &c., and hence those who may find profit in its pages are numerous throughout this as well as other countries. It will mark the progress of this department of science over the world, and each number, therefore, will be laden with new truths from the most recent studies of animal life at home and abroad.—*American Journal of Science and Arts*, vol. x., p. 287, 2d Series.

12. *External Symptoms of Starvation, as observed in the Famine Districts of Ireland:*—

"In grown-up persons, besides an amount of attenuation which seems to have absorbed all appearance of flesh or muscle, and to have left the bones of the frame barely covered with some covering which has but little semblance to anything we would esteem to be flesh; the skin of all the limbs assumes a peculiar character,—it is rough to the touch, very dry, and, did it not hang in places in loose folds, would be more of the nature of parchment than anything else with which I can compare it; the eyes are much sunk into the head, and have a dull painful look; the shoulder-bones are thrown up so high that the column of the neck seems to have sunk, as it were, into the chest; the face and head, from the wasting of the flesh, and the prominence of the bones, have a skull-like appearance; the hair is very thin upon the head; there is over the countenance a sort of pallor, quite distinct from that which utter decline of physical power generally gives in those many diseases in which life still continues after the almost entire consumption of the muscular parts of the body. In the case of the starved young, and we saw many hundreds, there are two or three most peculiar characteristic marks which distinguish them from the victims of other mortal ills; the hair on a starved child's head becomes very thin, often leaves the head in patches, and what there is of it stands up from the head; over the whole brow, in many instances, over the temple in almost all, a thick downy sort of hair grows, sometimes so thickly as to be quite palpable to the touch; between the fingers there are sores, very often there is anasarcous swelling of the ancles. In the majority of famine cases there is either dysentery or chronic diarrhœa."

Such is to-day, drawn in no exaggerated colours, the condition of Connaught. The devastation had been long preparing, and it is complete.—*Times*, *Tuesday*, *Sept.* 24, 1850.

ARTS.

13. *Detonating Sugar.*—In the meeting of the Royal Academy of Turin, for 31st Jannary 1849, Professor Ascanio Sobrero announced his discovery of a detonating sugar, obtained from that material, by means similar to the mode of preparing *Gun-cotton.*

Take pounded loaf-sugar and pour on it a mixture of two volumes of sulphuric acid (at Baruné 66), and one volume of nitric acid (at 43.) Immediately the sugar is converted into a tenaceous viscid substance, which is only partially dissoluble in the acids employed. On adding a large quantity of water (about twenty times that of the acids employed) the sugar is converted into a material with the following properties. It is very white; diffusible in the acid mixture, and absolutely insoluble in water, but very soluble in alcohol and sulphuric ether. When subject to a moderate heat, it melts, and is decomposed without detonation: but if suddenly heated to redness, it explodes like gun-powder, producing gaseous emanations, in which it is not difficult to recognise the Nitrous vapour and that of Cyanogen. By the blow of a hammer it also explodes, but feebly.

The composition of this fulminating sugar it will not be difficult to determine—more easily than that of gun-cotton—from its more slow decomposition under a graduated heat, when it may be exposed to the action of oxide of copper. Professor Sobrero, however, had not completed his analysis when his paper was read. Its composition may lead to the better understanding of other fulminating compounds, and we may be able to obtain fulminating substances by similar means from starch, flour, &c.—*Professor Thomas Stewart Traill.*

14. *An Ancient Art rediscovered.*—At a meeting of the Asiatic Society of London, some time ago, a human hand and a piece of beef, preserved by means of a preparation of vegetable tar, found on the borders of the Red Sea in the vicinity of Mocha, were presented; a specimen of the tar accompanied them. Colonel Holt, who presented the specimens, observes,—"During my residence on the Red Sea, a conversation with some Bedouin Arabs, in the vicinity of Mocha, led me to suspect that the principal ingredient used by the ancient Egyptians in the formation of mummies was nothing more than the vegetable tar of those countries, which is called by the Arabs, Katren. My first trials to prove the truth of this conjecture were on fowls and legs of mutton, and, though made in July, when the thermometer ranged at 94° in the shade, they succeeded so much to my satisfaction, that I forwarded some to England; and have now the pleasure to send to the society a human hand prepared in a similar way four years since. The best informed among the Arabs think that large quantities of camphor, myrrh, aloes, and frankincense were used in the preparation of the mummies. These specimens will, however, prove that such additions were by no means necessary, as the tar applied alone penetrates and discolours the bone. This tar is obtained from the branches of a small tree or shrub, exposed to a considerable degree of heat, and it is found in most parts of Syria and Arabia Felix."

NEW PUBLICATIONS.

1. An Introduction to Conchology, or Elements of the Natural History of Molluscous Animals: illustrated with one hundred and twelve woodcuts. One vol. 8vo, pp. 613. By George Johnston, M.D., LL.D. London: John Van Voorst, 1850. *A volume on scientific and popular Conchology by so accurate, experienced, and learned a naturalist as Dr Johnston, cannot but be acceptable to the numerous cultivators of this beautiful branch of Zoology. We have been instructed by its ample zoological and economical details, and truly gratified by its sound general views. The varied historical information communicated by the author, reminds us vividly of bygone times, and adds a charm to a volume which, we doubt not, will find a place in every zoological library, and be prized by the higher class of general readers.*

2. The Principles of Geology Explained, and viewed in their Relations to Revealed and Natural Religion. By the Rev. David King, LL.D. Second Edition, 12mo, pp. 286. Johnstone and Hunter, Edinburgh, 1850. *An agreeable and pleasantly written popular view of Geology.*

3. The Atlas of Physical Geography. Constructed by Augustus Petermann, F.R.G.S. Imperial quarto, pp. 142. London: Wm. S. Orr & Co., 1850. *The distinguished favour with which the public received the important Physical Geography of Professor Berghaus of Berlin, and the no less successful reception given to the magnificent Physical Atlas of our intelligent townsman Mr Alex. Keith Johnston, gave rise, and that speedily, to other similar but less extensive works on the continent of Europe and also in this island. Of these, the best we have seen is that of the well-known and experienced geographer, Augustus Petermann, Esq. (now resident in London), which we recommend on account of its geographical accuracy and great beauty of execution.*

4. A Sketch of the Physical Structure of Australia, so far as it is at present known, with two Geological Maps. By J. Beete Jukes, M.A., F.G.S., late Naturalist of H.M.S. Fly. T. & W. Boone, 29 New Bond Street, London, 1850. *To those interested in the geology of Australia we particularly recommend this volume, by Mr Jukes, a gentleman very favourably and generally known as an enterprising traveller and judicious observer. The accompanying coloured geological maps add to the value of the work.*

5. An Introduction to the Atomic Theory. By Charles Daubeny, M.D., F.R.S.L. Second edition, greatly enlarged. Oxford: At the University Press, 1850. *Dr Daubeny has, in this edition of his work on the Atomic Theory, added much to interest the chemist and general reader.*

6. Geognostische Briefe aus den Alpen. Von Bernard Cotta (Letters from the Alps.) Leipzig: J. O. Weigel, 1850. *These interesting and instructive Letters, by the present occupant of the Geognostical Chair of Werner, at Freyberg, independently of incidents of travel, give us an agreeable and generally accurate account of the Geognosy of the Alps, a subject of high importance as bearing on the general geology of Europe. They shew that the colossal mountain groups forming the European Alps exhibit, as we long ago pointed out, a structure very different from that of the Alps of Scotland. Ere long we trust to see British geologists occupied in working out the true geology of the Caledonian Alps. In this important investigation they will be materially assisted by these Letters of Professor Cotta—a translation of which we believe would be welcomed by our geologists.*

7. Graptolites de Bohème, par Joachim Barrande. Extrait du Système Silurien du Centre de la Bohème. 8vo, pp. 74, with plates. Prague, 1850. *Our excellent friend, M. J. Barrande, whose important geological investigations in the Silurian (Transition) districts of Bohemia, so deeply interesting to geologists, has published, under the above title, as an extract from his forthcoming great work on Bohemia, a very excellent account of those curious fossil animals the Trilobites. This account we recommend to the particular attention of the geologists of Scotland, as similar organic remains have been met with in Dumfriesshire, Peeblesshire, and Kirkcudbrightshire. The valuable account of M. Barrande's work, by our former pupil and friend Professor Nicol of Cork, contained in the present number of the Philosophical Journal, will enable our readers to judge in a general way of the important information expected from the "Systeme Silurien" of M. Barrande.*

List of Patents granted for Scotland from 16*th September to* 26*th December* 1850.

1. To Thomas Coats, of Ferguslie, in the town of Paisley, and county of Renfrew, in that part of Great Britain called Scotland, thread-manufacturer, "certain improvements in turning, cutting, and shaping wood and other materials."—23d September 1850.

2. To William Benson Stones, of Golden Square, in the county of Middlesex, Manchester warehouseman, "improvements in treating peat and other carbonaceous and ligneous matters so as to obtain products therefrom."—23d September 1850.

3. To Evan Leigh, of Miles Platting, near Manchester, in the county of Lancaster, cotton-spinner, "certain improvements in machinery or apparatus for preparing and spinning cotton and other fibrous substances."—25th September 1850.

4. To Joseph Wheeler Rogers, of Dublin, civil-engineer, "certain improvements in the preparation of peat, and in the manufacture of the same into fuel and charcoal."—30th September 1850.

5. To Jesse Bridgman, of London, gentleman, "certain improvements in separating the fatty and oily from the membranous portions of animal and vegetable substances."—30th September 1850.

6. To Richard Prosser, of Birmingham, in the county of Warwick civil-engineer, "improvements in machinery and apparatus for manufacturing metal tubes; which improvements in machinery are in part applicable for other purposes where pressure is required; also for improvements in the mode of applying metal tubes in steam-boilers or other vessels, and in the mode of cleaning out the tubes of steam-boilers, and in the mode of feeding or supplying steam-boilers with water."—1st October 1850.

7. To William Keates, of Liverpool, in the county of Lancaster, merchant, "improvements in machinery for manufacturing rollers and cylinders used for calico printing and other purposes."—4th October 1850.

8. To James Young, of Manchester, in the county of Lancaster, manufacturing-chemist, "certain improvements in the treatment of certain bituminous mineral substances, and in obtaining products therefrom."—7th October 1850.

9. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in dyeing yarn and in manufacturing certain woven fabrics;" being a communication.—8th October 1850.

10. To William Tudor Mabley, of Manchester, in the county of Lancaster, patent agent, "certain improvements in the manufacture of soap;" being a communication.—10th October 1850.

11. To Cuthbert Dinsdale, of Newcastle-upon-Tyne, dentist, "improvements in the manufacture of artificial palates and gums, and in the mode of setting or fixing natural and artificial teeth."—11th October 1850.

12. To John Beattie, of Liverpool, in the county of Lancaster, engineer, "certain improvements in steering vessels."—14th October 1850.

13. To John Grant, of Hyde Park Street, in the county of Middlesex, "improvements in heating and regulating temperature."—14th October 1850.

14. To Etienne Joseph Hanon Valcke, of the kingdom of Belgium, miller, "improvements in grinding."—14th October 1850.

15. To John Mercer, of Oakenshaw, within Clayton-le-Moors, in the county of Lancaster, gentleman, "improvements in the preparation of cotton and other fabrics and fibrous materials."—14th October 1850.

16. To William Erskine Cochrane, of Cambridge Terrace, Regent's Park, in the county of Middlesex, and Henry Francis, of Princes Street, Rotherhithe, "improvements in propelling, steering, and ballasting vessels, in the pistons of steam-engines, in fire-bars of furnaces, and in sleepers of railways."—14th October 1850.

17. To Alexander Dixon, of Abercorn Foundry, Paisley, "improvements in moulding iron and other metals."—16th October 1850.

18. To William Palmer, of Sutton Street, Clerkenwell, manufacturer, "improvements in the manufacture of candles and wicks."—16th August 1850.

19. To Engine Ablon, of Panton Street, Haymarket, in the county of Middlesex, "improvements in increasing the draft in the chimneys of locomotive and other engines;" being a communication.—16th October 1850.

20. To William Henry Green, of Basinghall Street, in the city of London, gentleman, "improvements in the preparation of peat, and in the mode of converting and applying some of the products derived thereby to the preservation of substances which are subject to decay."—16th October 1850.

21. To Charles Bury, of Salford, in the county of Lancaster, manager, "certain improvements in machinery or apparatus for cleaning, spinning, doubling, and throwing raw silk."—18th October 1850.

22. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, "improvements in manufacturing yarns;" being a communication.—18th October 1850.

23. To John Percy, of Birmingham, in the county of Warwick, doctor of medicine, and Henry Wiggin, of Birmingham aforesaid, manufacturer, "a new metallic alloy or new metallic alloys."—21st October 1850.

24. To Etienne Masson, of Place St Michel at Paris, gardener to the Central Society of Horticulture of France, "improvements in the preparation of certain vegetable alimentary substances, for the provisioning of ships and armies, and other purposes where the said substances are required to be preserved."—25th October 1850.

25. To Zachariah Morley, Esq., of Regent's Park, in the county of Middlesex, "certain improvements in the means or methods of, or apparatus or machinery for, decomposing water, and applying the products to useful purposes;" being a communication.—28th October 1850.

26. To Robert Lucas, of 3 Furnival's Inn, in the county of Middlesex, mechanical draughtsman, "improvements in telegraphic and printing apparatus;" being a communication.—31st October 1850.

27. To George Michiels, of London, gentleman, "improvements in treating coal, and in the manufacture of gas, and also in apparatus for burning gas;" being partly a communication.—5th November 1850.

28. To William Henry Ritchie, of Kennington, in the county of Surrey, gentleman, "improvements in stoves;" being a communication.—6th November 1850.

29. To Peter Spence, of Pendleton, Manchester, manufacturing chemist, "improvements in the manufacture of alum and certain alkaline salts, and in the manufacture of cement, part of which improvements are applicable in obtaining volatile liquids."—7th November 1850.

30. To Alfred George Anderson, of Great Suffolk Street, in the county of Surrey, soap-manufacturer, "improvements in the treatment of a substance produced in soap-making, and its application to useful purposes."—7th November 1850.

31. To John M'Nicoll, of Liverpool, in the county of Lancaster, engineer, "improvements in machinery for raising and conveying weights." —7th November 1850.

32. To John Lienan junior, of Wharf Road, City Road, in the county of Middlesex, merchant, "improvements in purifying or filtering oils and other liquids."—7th November 1850.

33. To John Tatham and David Cheetham, of Rochdale, in the county of Lancaster, machine-makers, "certain improvements in the manufacture of cotton and other fibrous material, and fabrics composed of such materials"—7th November 1850.

34. To Hugh Mair, of Ingram Street, in the city of Glasgow, North Britain, manufacturer, "improvements in certain classes of figured muslins, and in the machinery or apparatus employed in the manufacture or production thereof, which improvements, or parts thereof, are applicable to harness-weaving generally."—11th November 1850.

35. To George Hurwood, of Ipswich, in the county of Suffolk, engineer, "improvements in grinding corn and other substances."—11th November 1850.

36. To Evan Protheroe, of Austin Friars, in the city of London, merchant, "improvements in the manufacture of oxide of zinc, and in making paints from oxide of zinc;" being a communication from Stanilas Tranquille Modeste Sorel, of Paris, engineer.—11th November 1850.

37. To Rodolphe Helbronner, of Regent Street, in the county of Middlesex, "improvements in preventing the external air and dust and noise from entering apartments;" being a communication.—11th November 1850.

38. To James Samuel, of Willoughby House, in the county of Middlesex, civil-engineer, "certain improvements in the construction of railways and steam-engines, and in steam-engine machinery."—12th November 1850.

39. To Theodore Cartali, of Manchester, in the county of Lancaster, merchant, "certain improvements in the treatment or preparation of yarns or threads for weaving, and in the manufacture of certain woven fabrics;" being a communication.—13th November 1850.

40. To John Clare junior, of Exchange Buildings, Liverpool, gentleman, "improvements in the manufacture of metallic casks."—13th November 1850.

41. To Charles Bury, of Salford, in the county of Lancaster, manager, "certain improvements in machinery or apparatus for preparing and spinning, doubling, or twisting, silk-waste, cotton-wool, flax, or other fibrous substanccs."—13th November 1850.

42. To Richard Clyburn, engineer, of the firm of D. M'Lean and Son, of St George Street East, in the county of Middlesex, "improvements in wheel-carriages;" being a communication.—14th November 1850.

43. To John Tucker, of the Royal Dockyard, Woolwich, in the

county of Kent, shipwright, "improvements in steam-boilers, and in gearing, cleansing, and propelling vessels;" being a communication.—15th November 1850.

44. To John Robert Johnson, of Crawford Street, in the county of Middlesex, chemist, "improvements in fixing colours on fabrics made of cotton or other fibre."—15th November 1850.

45. To Clement Augustus Kurtz, of Manchester, in the county of Lancaster, practical chemist, "improvements in dyeing;" being a communication.—15th November 1850.

46. To Antoine Pauwels, of Paris, France, merchant, and Vincent Dubochet, also of Paris, France, merchant, "certain improvements in the production of coke and of gas for illumination, and also in regulating the circulation of such gas."—18th November 1850.

47. To Robert Cotgreave, of Eccleston, in the county of Chester, farmer, "certain improvements in machinery or apparatus for draining and cultivating land."—19th November 1850.

48. To Alexander Mein, accountant in Glasgow, trustee upon the sequestrated estate of the late James Smith of Deanston, North Britain, "certain improvements in treating the fleeces of sheep when on the animals," which improvements were the invention of the said late James Smith of Deanston.—20th November 1850.

49. To John Hamilton, of Princes Square, Glasgow, and John Weems of Johnstone, in the kingdom of Scotland, "improvements in warming and ventilating buildings and structures."—20th November 1850.

50. To John Turner, of Birmingham, in the county of Warwick, engineer, and Joseph Hardwick, of Birmingham aforesaid, builder, "a certain improvement or certain improvements in the construction and setting of steam-boilers."—20th November 1850.

51. To William and Colin Mather, of Salford, engineers, and Ferdinand Haselowsky, of Berlin, in the kingdom of Prussia, engineer, "improvements in machinery for washing, steaming, drying, and finishing cotton, linen, and woollen fabrics."—21st November 1850.

52. To John Matthews, of Kidderminster, foreman, "improvements in sizing paper."—22d November 1850.

53. To William Radley, chemical engineer, and Frederick Meyer, oil merchant, both of Lambeth, in the county of Surrey, "improvements in treating fatty oleaginous, resinous, bitumenous and cerous bodies in the manufacture and application of them, and of their compounds and subsidiary products, together with the apparatus to be employed therein to new and other useful purposes."—22d November 1850.

54. To Edwin Pettitt, of Birmingham, in the county of Warwick, civil-engineer, "improvements in the manufacture of glass in the method of forming or shaping and ornamenting vessels and articles of glass, and in the construction of furnaces and annealing kilns."—22d November 1850.

55. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in the preparation and manufacture of caoutchouc or India rubber;" being a communication.—27th November 1850.

56. To Jules Le Bastier, of Paris, in the Republic of France, but temporary of Frankfort-on-the-Main in Germany, gentleman, "certain improvements in machinery or apparatus for printing."—27th November 1850.

57. To Isac Lewis Pulvermacher, of Vienna, engineer, "improvements in galvanic batteries, in electric telegraphs, and in electro-magnetic and magneto-electric machines."—28th November 1850.

58. To Guillaume Ferdinand de Douhet, of Clermont Ferrand, in the Republic of France, gentleman, "certain improvements in the dis-oxygenation, and the mutual reoxygenation of certain bodies, and the application of the products therefrom, either separately or simultaneously employed to various useful purposes."—28th November 1850.

59. To George Benjamin Thorneycroft, of Wolverhampton, in the county of Stafford, iron-master, "improvements in the manufacture of crank axles."—2d December 1850.

60. To David Napier, and James Murdoch Napier, of the York Road, Lambeth, in the county of Surrey, engineers, "improvements in apparatus for separating fluid from other matter."—2d December 1850.

61. To David Auld, of the City of Glasgow, North Britain, engineer, "certain improvements in steam-engines, and in the working of steam-boilers or generators, and in apparatus connected therewith."—2d December 1850.

62. To Jean Aime Marnas, of Lyons, "improvements in the manufacture of indigo;" being a communication.—2d December 1850.

63. To Peter Wood, of the Firm of Thomas Bury and Company, dyers, calenderers, and finishers, Adelphi Works, Salford, in the county of Lancaster, "improvements in figuring and ornamenting woven fabrics and paper, and in machinery employed therein."—4th December 1850.

64. To William Melville, of Roebank Works, Lochwinnoch, in the county of Renfrew, North Britain, calico printer, "certain improvements in weaving and manufacturing, and printing carpets and other fabrics."—6th December 1850.

65. To Peter Armand Le Comte de Fontainemoreau, 4 South Street, Finsbury, London, and also of 24 Boulevart Poissonierre, Paris, in France, patent agent for inventions, "certain improvements in oscillating engines;" being a communication.—7th December 1850.

66. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "an improved composition applicable to the coating of wood, metals, plaster, and other substances which are required to be preserved from decay, which composition may be also employed as a pigment or paint;" being a communication.—9th December 1850.

67. To Thomas Deakin, of Balsall Heath, in the county of Worcester, Esquire, "certain improvements in rolling metals, and in the manufacture of metal tubes, also in apparatus and machinery in connection therewith."—11th December 1850.

68. To John George Taylor, of the city of London, merchant, "certain improvements in dress and other pins, and in other dress fastenings and ornaments."—11th December 1850.

69. To Robert Olddiss Bancks, of the firm of Bancks Brothers, of Weirhouse Mill, Chesham, in the county of Bucks, and 20 Piccadilly, London, paper makers and card makers, "improvements in the manufacture of paper."—13th December 1850.

70. To George Edward Derring, Lockleys, in the county of Herts, Esquire, "improvements in the means of, and apparatus for, communicating intelligence by electricity."—17th December 1850.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Biographical Sketch of the late Robert Stevenson, Esq., F.R.S.E., Member of the Wernerian Natural History Society, Fellow of the Geological Society, &c., Civil Engineer. By his Son ALAN STEVENSON, LL.B., F.R.S.E., Civil Engineer. Communicated by the Author.*

ROBERT STEVENSON was born at Glasgow on the 8th June 1772, and died at Edinburgh on the 12th July 1850, in the seventy-ninth year of his age. His father Alan Stevenson was a partner in a West India House in Glasgow, and died in the Island of St Christopher, while on a visit to his brother, who managed the business abroad. His only son Robert, the subject of this memoir, was then an infant; and, with his mother, was ultimately left in circumstances of the greatest difficulty, for the same epidemic fever which deprived him of his father carried off his uncle also, at a time when his loss operated most disadvantageously on the interests of the business which he had superintended. In this manner my father's early education was conducted, although, as the sequel shews, with tolerable success, yet under circumstances which could not by any means be called favourable. This success was chiefly due to the energy of his mother, Jane Lillie, who was a woman of great prudence and remarkable fortitude, based on deep convictions of religion. It appears from some memoranda left by my father, for the information of his family, that his mother had intended him for the sacred ministry, with a view to which he had been sent to the school of a famous linguist of his day,

* Read before the Royal Society of Edinburgh, on 17th February 1851.

Mr Macintyre. Circumstances, however, occurred which entirely changed his prospects and pursuits. Soon after he had attained his fifteenth year, his mother was married to Mr Thomas Smith, an ingenious man, who had commenced life as a tinsmith in Edinburgh, and who afterwards directed his attention to the subject of Lighthouses. So successful were Mr Smith's endeavours to improve the mode of illumination, by substituting oil lamps with parabolic mirrors for the open coal fires, which formerly served for beacons to the mariner, that he obtained the patronage of Professor Robison and Sir David Hunter Blair, and was appointed engineer to the Lighthouse Board, immediately after its constitution by the Act of 1786. In these pursuits, my father had rendered himself useful to Mr Smith, who entrusted him, at the early age of nineteen, with the superintendence of the erection of a lighthouse on the Island of Little Cumbrae in the River Clyde, according to a plan which he had furnished to the Trustees for the Clyde Navigation. This connection soon led to his adoption as Mr Smith's partner in business, and in 1799 to his marriage with his eldest daughter; and as the entire management of the lighthouse business had already for some years, with the concurrence of the Board, devolved upon him, he naturally succeeded Mr Smith as engineer, an office which he resigned in 1843, after having fulfilled its arduous duties for about half a century.

During the cessation of the works at Cumbrae in winter, Mr Stevenson, who, even at that time, had determined to follow out the profession of a civil engineer, and had began to feel the want of systematic training, applied himself with great zeal to the physical sciences in the Andersonian Institution at Glasgow, as well as to the mathematics, the practice of surveying, and to architectural and mechanical drawing. Of the kindness of Dr Anderson, who presided over that institution, he ever entertained a most grateful remembrance, and often spoke of him as one of his best advisers and kindest friends. In the manuscript memoranda already noticed, he thus records his obligations to Dr Anderson. It was "the practice of Professor A. kindly to befriend and forward the views of his pupils; and his attention to me during the few years I had the pleasure of being known to him, was of a very marked kind, for he directed my attention to various pursuits, with the view to my coming forward as an engineer."

After completing the Cumbrae Lighthouse, he was engaged under Mr Smith in erecting lighthouses on the Pentland Skerries in Orkney; and notwithstanding his active duties in summer, he was so zealous in the pursuit of knowledge that he contrived during several successive winters to attend the philosophical classes at the University of Edinburgh. In this manner he attended Professor Playfair's second and third mathematical courses, two sessions of Professor Robison's natural philosophy, two courses of chemistry under Dr Hope, and two of natural history under Professor Jameson. To these he added a course of moral philosophy under Dugald Stewart, and also a course of logic, and one of agriculture. "I was prevented, however," he remarks in the manuscript memoranda, "from taking my degree of M.A. by my slender knowledge of Latin, in which my highest book was the *Orations of Cicero*, and by my total want of Greek." Such zeal in the pursuit of knowledge under so many discouragements, and views so enlarged of the benefits and value of a liberal education, are characteristics of a mind of no ordinary vigour.

The great work of Mr Stevenson's life, and that on which his reputation as an engineer principally rests, is the Bell Rock Lighthouse. Of the progress of that great undertaking, he has left a lasting memorial and most interesting narrative in his "Account," a quarto volume of upwards of 500 pages, illustrated with numerous plates. But there are some circumstances connected with the early history of that work which, while they could not properly have found a place in his own narrative, have been noticed in the above-mentioned manuscript memoranda, from which I shall transcribe a few paragraphs detailing his early efforts and disappointments while designing that lighthouse:—

"All knew the difficulties of the erection of the Eddystone Lighthouse, and the casualties to which that edifice had been liable; and in comparing the two situations, it was generally remarked that the

Eddystone was barely covered by the tide at *high water*, while the Bell Rock was barely uncovered at *low water*.

"I had much to contend with in the then limited state of my experience; and I had in various ways to bear up against public opinion as well as against interested parties. I was in this state of things, however, greatly supported, and I would even say often comforted, by Mr Clerk of Eldin, author of the System of Breaking the Line in Naval Tactics. Mr Clerk took great interest in my models, and spoke much of them in scientific circles—he carried men of science and eminent strangers to the model-room which I had provided in Merchants' Hall, of which he sometimes carried the key, both when I was at home and while I was abroad. He introduced me to Lord Webb Seymour, to Admiral Lord Duncan, and to Professors Robison and Playfair, and others. Mr Clerk had been personally known to Smeaton, and used occasionally to speak of him to me."

It is impossible to read this little narrative without feeling a respect for Mr Clerk's hearty enthusiasm, and perceiving the beneficial influence which a kindly disposition, when thus united with an active and inventive mind like his, is calculated to produce on the prospects and pursuits of a young man, by stimulating an honourable emulation and discouraging a desponding spirit.

"But at length," the memorandum continues, "all difficulties with the public as well as with the better informed few, were dispelled by the fatal effects of a dreadful storm from the NE., which occurred in December 1799, when it was ascertained that no fewer than seventy sail of vessels were stranded or lost, with many of their crews, upon the coast of Scotland alone! Many of them, it was not doubted, might have found a safe asylum in the Firth of Forth, had there been a lighthouse upon the Bell Rock, on which, indeed, it was generally believed the *York*, of 74 guns, with all hands perished, none being left to tell the tale! The coast for many miles exhibited portions of that fine ship. There was now, therefore, but one voice. 'There must be a lighthouse erected on the Bell Rock.'

"Previous to this dreadful storm I had prepared my pillar-formed model, a section of which is shewn in Plate VII. of the '*Account of the Bell Rock Lighthouse*.' Early in the year 1800, I for the first time landed on the rock to see the application of my model to the situation for which it was designed and made. On this occasion I was accompanied by my friend Mr James Haldane, architect, my draughtsman, and whose pupil I had been for architectural drawing. Our landing was at low water of a spring tide, when a good *space* of rock was above water. I had no sooner set foot upon the rock than

I laid aside all idea of a pillar-formed structure, fully convinced that a building on similar principles with the Eddystone would be found practicable. On my return from this visit to the rock, I immediately set to work in good earnest with a design of a stone lighthouse, and modelled it. Of this design a section is also given in Plate VII. above noticed. I accompanied this design with a report or memorial to the Lighthouse Board, which I gave in the Appendix of my 'Account' at p. 440. The pillar-formed plan I estimated at £15,000, and the stone building at £42,000.* But still I found that I had not made much impression on the Board on the score of expense, for they feared it would cost much more than forty or fifty thousand pounds. Here, therefore, the subject rested with the Board for a time.

"To the very last the bankers were in doubt as to their security on the dues for so great and hazardous an undertaking; and the bill included an authority to borrow £25,000 from the Exchequer. I attended this bill through Parliament. Mr Rennie and myself were examined; but the only plans and information otherwise before the Committee were those already noticed, which I had laid before the Board in 1800. It was not at that time required by the standing orders to produce plans in a case of this kind.

"I had no sooner returned from London, after the passing of the Act, than I received orders from the Board to procure a Light-Ship, and to take other steps preparatory to the commencement of the work. The Act contained a provision, that as soon as a Light-Ship was moored, and due intimation given of the exhibition of a light from it, the collection of *half-duties* should commence,—a suggestion which was made while the bill was in draft by Mr Charles Cuningham, the secretary of the Board.

"The Lighthouse Act having obtained the royal assent, I began to feel a new responsibility. The erection of a lighthouse on a rock, about twelve miles from land, and so low in the water, that the foundation course must be at least on a level with the lowest tide, was an enterprise so full of uncertainty and hazard, that it could not fail to press on my mind. I felt regret that I had not had the opportunity of a greater range of practice to fit me for such an undertaking. But I was fortified by an expression of my friend Mr Clerk, in one of our conversations upon its difficulties. 'This work,' said he, 'is unique, and can be little forwarded by experience of ordinary masonic operations. In this case, Smeaton's Narrative must be the text book, and energy and perseverance *the pratique*.'"

How well Mr Stevenson met the demands on his perseverance, fortitude and self-denial, which, in the course of his great enterprise, were made on him, the history of the operations,

* The actual cost was £61,331, 9s., 2d., as shewn at p. 483 of the "Account."

and their successful completion, abundantly show. The work was, indeed, in all respects, peculiarly suited to his tastes and habits; and Mr Clerk truly, although perhaps unconsciously, characterised the man, in his terse statement of what would be required of him. No one can read his account of the Bell Rock Lighthouse without perceiving the justness of this estimate of his character. His daily cheerful participation in all the toils and hazards which were, for two seasons, endured in the floating Light-Ship, and afterwards in the Timber-House or Beacon, over which the waves broke with prodigious force, and caused a most alarming twisting movement of its main supports, were proofs not merely of calm and enduring courage, but of great self-denial and enthusiastic devotion to his calling. On one occasion in particular, his fortitude and presence of mind were most severely tried, and well they stood the test. I shall give the narrative of this most interesting adventure in his own words; but I cannot do so without expressing the regret I have so often felt, that, from some mistaken delicacy, he had been induced throughout his "Account," to speak of himself in the third person as "the writer." This has encumbered the style with artificial phraseology, has damped the ardour of the narrator, and in some instances has led to an awkward ambiguity. Yet, notwithstanding those defects, the following passage possesses great interest:—

"Soon after the artificers landed they commenced work; but the wind coming to blow hard, the Smeaton's boat and crew, who had brought their complement of eight men to the rock, went off to examine her riding ropes, and see that they were in proper order. The boat had no sooner reached the vessel than she went adrift, carrying the boat along with her; and both had even got to a considerable distance before this situation of things was observed, every one being so intent upon his own particular duty that the boat had not been seen leaving the rock. As it blew hard, the crew, with much difficulty, set the mainsail upon the Smeaton, with a view to work her up to the buoy, and again lay hold of the moorings. By the time that she was got round to make a tack towards the rock, she had drifted at least three miles to leeward, with the boat astern; and having both the wind and a tide against her, the writer perceived, with no little anxiety, that she could not possibly return to the rock till long after its being overflowed; for, owing to the anomaly of the tides, formerly noticed, the Bell Rock is completely under water before the ebb abates to the offing.

" In this perilous predicament, indeed, he found himself placed between hope and despair ; but certainly the latter was by much the most predominant feeling of his mind,—situate upon a sunken rock in the middle of the ocean, which, in the progress of the flood tide, was to be laid under water to the depth of at least twelve feet in a stormy sea. There were this morning in all thirty-two persons on the rock, with only two boats, whose complement, even in good weather, did not exceed twenty-four sitters ; but to row to the floating light with so much wind, and in so heavy a sea, a complement of eight men for each boat was as much as could with propriety be attempted, so that in this way about one-half of our number was unprovided for. Under these circumstances, had the writer ventured to despatch one of the boats, in expectation of either working the Smeaton sooner up towards the rock, or in hopes of getting her boat brought to our assistance, this must have given an immediate alarm to the artificers, each of whom would have insisted upon taking to his own boat, and leaving the eight artificers belonging to the Smeaton to their chance. Of course, a scuffle might have ensued, and it is hard to say, in the ardour of men contending for life, where it might have ended. It has even been hinted to the writer, that a party of the *pickmen* were determined to keep exclusively to their own boat against all hazards.

" The unfortunate circumstance of the Smeaton and her boat having drifted was, for a considerable time, only known to the writer, and to the landing-master, who removed to the further point of the rock, where he kept his eye steadily upon the progress of the vessel. While the artificers were at work, chiefly in sitting or kneeling postures, excavating the rock, or boring with the jumpers, and while their numerous hammers, and the sound of the smith's anvil, continued, the situation of things did not appear so awful. In this state of suspense, with almost certain destruction at hand, the water began to rise upon those who were at work on the lower parts of the sites of the beacon and lighthouse. From the run of sea upon the rock, the forge fire was also sooner extinguished this morning than usual, and the volumes of smoke having ceased, objects in every direction became visible from all parts of the rock. After having had about three hours' work, the men began, pretty generally, to make towards their respective boats for their jackets and stockings, when, to their astonishment, instead of three they found only two boats, the third being adrift with the Smeaton. Not a word was uttered by any one, but all appeared to be silently calculating their numbers, and looking to each other with evident marks of perplexity depicted in their countenances. The landing-master, conceiving that blame might be attached to him for allowing the boat to leave the rock, still kept at a distance. At this critical moment, the author was standing upon an elevated part of Smith's Ledge, where he endeavoured to mark the progress of the Smeaton, not a little surprised that the crew did not cut the praam adrift, which greatly retarded her way, and amazed that some effort was not making to bring at least the boat, and attempt our relief. The workmen looked stedfastly upon the writer, and turned occasionally towards the vessel, still far to leeward. All this passed in the most perfect silence, and the melancholy solemnity of the group made an impression never to be effaced from his mind.

"The writer had all along been considering various schemes, providing the men could be kept under command, which might be put in practice for the general safety, in hopes that the Smeaton might be able to pick up the boats to leeward, when they were obliged to leave the rock. He was, accordingly, about to address the artificers on the perilous nature of their circumstances, and to propose that all hands should unstrip their upper clothing, when the higher parts of the rock were laid under water; that the seamen should remove every unnecessary weight and incumbrance from the boats; that a specified number of men should go into each boat, and that the remainder should hang by the gunwales, while the boats were to be rowed gently towards the Smeaton, as the course to the Pharos or Floating Light lay rather to windward of the rock. But when he attempted to speak, his mouth was so parched, that his tongue refused utterance, and he now learned by experience that the saliva is as necessary as the tongue itself for speech. He then turned to one of the pools on the rock and lapped a little water, which produced an immediate relief. But what was his happiness, when, on rising from this unpleasant beverage, some one called out 'a boat! a boat!' and on looking around at no great distance, a large boat was seen through the haze making towards the rock. This at once enlivened and rejoiced every heart. The timeous visitor proved to be James Spink the Bell Rock pilot, who had come express from Arbroath with letters. Every one felt the most perfect happiness at leaving the Bell Rock this morning, though a very hard and even dangerous passage to the floating light still awaited us, as the wind by this time had increased to a pretty hard gale, accompanied with a considerable swell of sea. The boats left the rock about nine, but did not reach the vessel till twelve o'clock noon, after a most disagreeable and fatiguing passage of three hours. Every one was as completely drenched in water as if he had been dragged astern of the boats."

The state of suffering and discomfort as well as danger on board the floating light, which lay moored off the rock during the two first seasons of the work, before the timber barrack was used as a habitation, is described in the following passage, which presents a striking idea of the continual anxiety that must have existed in the minds of those engaged in the work, and of the frequent calls for energetic and courageous exertion.

"About two o'clock P.M. a great alarm was given throughout the ship, from the effects of a very heavy sea which struck her, and almost filled the waist, pouring down into the berths below, through every chink and crevice of the hatches and sky-lights. From the motion of the vessel being thus suddenly deadened or checked, and from the flowing in of the water above, it is believed there is not an individual on board who did not think, at the moment, that the vessel had foundered and was in the act of sinking. On deck there was only one solitary individual looking out, to give the alarm, in the event of the ship making from her moorings. The seaman on watch continued only two hours; he had no great-coat or overall of any kind, but was simply dressed in his ordinary jacket and trousers; his hat was tied under his chin with a napkin, and he stood aft the foremast to which he had lashed himself with a gushet or small rope round his waist, to prevent his falling upon deck or being washed overboard. Upon deck everything that was moveable was out of sight, having either been stowed below, previous to the gale, or been washed overboard. Some trifling parts of the quarter boards were damaged by the breach of the sea, and one of the boats upon deck was about one-third full of water, the oyle-hole or drain having been accidentally stopped up, and part of the gunwale had received considerable injury. Although the previous night had been a very restless one, it had not the effect of inducing sleep in the writer's berth on the succeeding one; for having been so much tossed about in bed during the last thirty hours, he found no easy spot to turn to, and his body was all sore to the touch, which ill accorded with the unyielding materials with which his bed-place was surrounded.

"This morning about eight o'clock the writer was agreeably surprised to see the scuttle of his cabin sky-light removed, and the bright rays of the sun admitted. Although the ship continued to roll excessively, and the sea was still running very high, yet the ordinary business on board seemed to be going forward on deck. It was impossible to steady a telescope, so as to look minutely at the progress of the waves, and trace their breach upon the Bell Rock, but the height to which the cross-running waves rose in sprays, when they met each other, was truly grand, and the continued roar and noise of the sea was very perceptible to the ear. To estimate the height of the sprays at 40 or 50 feet would surely be within the mark. Those of the workmen who were not much afflicted with sea-sickness came upon deck, and the wetness below being dried up, the cabins were again brought into a comfortable state. Every one seemed to meet, as if after a long absence, congratulating his neighbour upon the return of good weather. Little could be said as to the comfort of the vessel, but after riding out such a gale, no one felt the least doubt or hesitation as to the safety and good condition of her moorings. The master and mate were extremely anxious however, to heave in the hempen cable, and see the state of the clinch or iron ring of the chain cable. But the vessel rolled at such a rate that the seamen could not possibly keep their feet at the windlass, nor work the hand-spokes, though it had been several times attempted since the gale took off.

"About twelve noon, however, the vessel's motion was observed to be considerably less, and the sailors were enabled to walk upon deck with some degree of freedom. But to the astonishment of every one it was soon discovered that the floating light was adrift! The windlass was instantly manned, and the men soon gave out that there was no strain upon the cable. The mizzen sail, which was bent for the occasional purpose of making the vessel ride more easily to the tide, was immediately set, and the other sails were also hoisted in a short time, when, in no small consternation, we bore away about one mile to the south-westward of the former station, and there let go the best bower-anchor and cable, in twenty fathoms water, to ride until the swell of the sea should fall, when it might be practicable to grapple for the moorings, and find a better anchorage for the ship.

"As soon as the deck could be cleared the cable-end was hove up, which had parted at the distance of about 50 fathoms from the chain moorings. On examining the cable, it was found to be considerably chafed, but where the separation took place, it appeared to be worn through, or cut shortly off. How to account for this would be difficult, as the ground, though rough and gravelly, did not, after much sounding, appear to contain any irregular parts. It was therefore conjectured that the cable must have hooked some piece of wreck, as it did not appear from the state of the wind and tide, that the vessel could have *fouled* her anchor, when she veered round with the wind, which had shifted in the course of the night from NE. to NNW.

"Be this as it may, it was a circumstance quite out of the power of man to prevent, as, until the ship drifted, it was found impossible to heave up the cable. But what ought to have been the feeling of thankfulness to that providence which regulates and appoints the lot of man, when it is considered that if this accident had happened during the storm, or in the night after the wind had shifted, the floating light must inevitably have gone ashore upon the Bell Rock. In short, it is hardly possible to conceive any case more awfully distressing than our situation would have been, or one more disastrous to the important undertaking in which we were engaged."

The Beacon or Barrack was a singular habitation, somewhat resembling a pigeon-house, perched on logs, on which the tide rose 16 feet in calm weather, and was exposed to the assault of every wave. Of the perils and discomforts of such a habitation, the following passage gives a lively picture:—

"The gale continues with unabated violence to-day, and the sprays rise to a still greater height, having been carried over the masonry of the building, or about 90 feet above the level of the sea. At four o'clock this morning it was breaking into the cook's berth, when he rung the alarm-bell, and all hands turned out to attend to their personal safety. The floor of the smith's or mortar gallery was now completely burst up by the force of the sea, when the whole of the deals and the remaining articles upon the floor were swept away, such as the cast-iron mortar-tubs, the iron hearth of the forge, the smith's bellows, and even his anvil, were thrown down upon the rock. The boarding of the cook-house, or storey above the smith's gallery, was also partly carried away, and the brick and plaster work of the fire-place shaken and loosened. At low water it was found that the chain of the moveable beam-crane at the western wharf had been broken, which set the beam at liberty, and greatly endangered the quay-ropes by its motion. It was observed, during this gale, that the beacon-house had a good deal of tremor, but none of that 'twisting motion' occasionally felt and complained of before the additional wooden struts were set up for the security of the principal beams; but this effect had more especially disappeared ever since the attachment of the great horizontal iron bars in connection with these supports, instead of the chain-braces shewn in Plate VIII. Before the tide rose to its full height to-day, some of the artificers passed along the bridge into the lighthouse, to observe the effects of the sea upon it, and they reported that they had felt a slight tremulous motion in the building, when great seas struck it in a certain direction about high water mark. On this occasion, the sprays were again observed to wet the balcony, and even to come over the parapet wall into the interior of the light-room. In this state of the weather, Captain Wilson and the crew of the 'Floating Light' were much alarmed for the safety of the artificers upon the rock, especially when they observed, with a telescope, that the floor of the smith's gallery had been carried away, and that the triangular cast-iron sheer-crane was broken down. It was quite impossible, however, to do anything for their relief until the gale should take off."

The great merit due to my father, as the architect of the Bell Rock Lighthouse, lies in his bold conception of, and confident unshaken belief in, the possibility of executing a tower of masonry on the Bell Rock, a situation undoubtedly, from the level of the rock, which is covered by every tide, of much greater difficulty than the Eddystone. But, while we chiefly admire his boldness and perseverance, his mechanical skill in carrying on the work is also deserving of high praise. Not only did he conceive the plan of the *jib* and *balance-*

cranes—which he afterwards applied, with much advantage, in the erection of the tower—but his zeal, ever alive to the possibility of improving on the conceptions of his great master, Smeaton, led him to introduce some advantageous changes in the arrangements of the masonry of the tower, some of which he has briefly described in the following passage:—

"The floor courses of the Bell Rock Lighthouse lay horizontally upon the walls, as will be seen from the sections in Plates VII. and XVI. They consisted in all of eighteen blocks, but only sixteen were laid in the first instance, as the centre stones were necessarily left out, to allow the shaft of the balance-crane to pass through the several apartments of the building. In the same manner also the stone which formed the interior side of the man-hole was not laid till after the centre stone was in its place and the masonry of the walls completed. The number of stones above alluded to are independently of the sixteen joggle pieces with which the principal blocks of the floors were connected, as shewn in the diagrams of Plates VII. and XIII. The floors of the Eddystone Lighthouse, on the contrary, were constructed of an arch form, and the haunches of the arches bound with chains, to prevent their pressing outward to the injury of the walls. In this, Mr Smeaton followed the construction of the domes of St Paul; and this mode might also be found necessary at the Eddystone, from the want of stones in one length to form the outward wall and floor, in the then state of the granite quarries of Cornwall. At Mylnefield quarry, however, there was no difficulty in procuring stones of the requisite dimensions; and the writer foresaw many advantages that would arise from having the stones of the floors to form part of the outward walls without introducing the system of arching. In particular, the pressure of the floors upon the walls would thus be perpendicular; for as the stones were prepared in the sides, with *groove and feather*, after the manner of the common house floor, they would, by this means, form so many girths, binding the exterior walls together, as will be understood by examining the diagrams and section of Plate VII., with its letterpress description; agreeably to which, he had modelled the floors in his original designs for the Bell Rock, which were laid before the Lighthouse Board in the year 1800."

The Commissioners entertained a high sense of his services at the Bell Rock Lighthouse; and as many of them took a deep interest in the whole course of that remarkable work, and paid occasional visits to see its progress, they were well able to appreciate the fidelity and zeal with which he devoted himself to this arduous task. Twelve years after the work was completed, it was moved by the late Sir William Rae, Baronet, then Lord Advocate of Scotland, at a meeting held in the lighthouse itself on the 19th July 1824—"That a bust of Mr Robert Stevenson be obtained and placed in the library of the Bell Rock Lighthouse, in testimony of the sense entertained by the Commissioners of his distinguished talent and indefatigable zeal in the erection of that lighthouse." A beautiful bust, by Samuel Joseph, was accordingly placed in that room, which, from its striking resemblance, recalls in a very pleasing manner the memory of my father, coupled with many of his counsels delivered on the spot during my frequent visits to the Bell Rock in his company.

It appears, from the Minutes of the Commissioners, that my father performed his first tour of inspection of the lighthouses, and made the Annual Report to the Board, in the year 1797. During the long period of his incumbency which followed, he designed and executed twenty-three lighthouses in the district of the Commission, most of them in situations which called for much forethought and great energy on the part of the executive officer. All his works were characterised by the same sagacity and comprehensive views, and exhibit successive stages of improvement, equally indicative of the growing prosperity of the Board, and the alacrity and zeal with which their engineer laboured in his vocation. In no country has the catoptric system of illuminating lighthouses been brought to so high a degree of perfection as in Scotland; and whether we consider the accuracy and beauty of the optical apparatus, the arrangements of the buildings, or the discipline observed by the light-keepers, we cannot fail to recognise the impress of that energetic and comprehensive cast of mind which directed the whole. With the strictest propriety, my father may be said to have created the lighthouse system of Scotland, and brought it to its present state of perfection. His merits indeed, in this respect, were generally acknowledged in other quarters; and many of the Irish lighthouses, and several lighthouses in our colonies were fitted up with apparatus prepared under his superintendence. In the course of his labours as engineer to the Lighthouse Board, his attention was much given to the subject of distinction among lights, a matter of the utmost importance in narrow seas, where many lights are visible at once. He was the inventor of two useful distinctions—the *intermittent* and *flashing* lights, for the latter of which he received from the late King of the Netherlands a gold medal, as a mark of his Majesty's approbation. In the first of those distinctions the light is suddenly obscured and as suddenly revealed to sight, at unequal intervals of time, in a manner which completely distinguishes it from the ordinary revolving light, which from darkness gradually increases in power till it reaches its brightest place, and then gradually declines until it is again obscured. The flashing light exhibits, by means of a rapid revolution of the frame which carries the lamps and a peculiar arrangement in their position, a sudden flash of great power, once in five seconds of time.

But while his reputation as an engineer naturally rests chiefly on his great work of the Bell Rock and his improvements in the illumination of lighthouses, he is by no means without other claims to be remembered. Besides his official duties, he took a considerable share in the general engineering of his day, and acted on many occasions in conjunction with Rennie and Telford, and afterwards with Messrs Walker and Cubitt, with all of whom he ever maintained a friendly intercourse. Soon after the peace in 1815 the public mind was naturally directed to the improvement of our internal resources, which the long continuance of the continental war had thrown unduly into the shade. Roads, bridges, harbours, canals, and railways, soon became topics of public attention and general interest; and my father's known sagacity and energy rendered him a useful adviser on many of those subjects. In the course of his professional life he designed and executed several important bridges, such those of Stirling, Marykirk, Annan, and the Hutcheson bridge over the Clyde at Glasgow. The beautiful approach to the City of Edinburgh from the east, by the Calton Hill, was designed by him, and executed under his direction ; and I mention this the more willingly, as many of our citizens are not aware to whom we owe this splendid entrance to our "own romantic town." He also surveyed and traced the lines of many canals and railways which have since been executed, more or less, in accordance with the advice contained in his numerous printed reports. I would especially mention his projected canal, and afterwards railway on one level between Edinburgh and Glasgow; his great Strathmore Canal and Railway, on one level, which would have connected the towns of Perth, Forfar, Arbroath, Montrose, and Brechin. There is scarcely a harbour or a navigation in Scotland about which at some time he has not given his valuable advice, which was also often called for in England and Ireland. His printed reports and contributions to engineering knowledge extend, when collected, to four thick quarto volumes; and during his long life of industry he did much, which, like a large portion of the labours of all professional men, was never known beyond the sphere immediately affected by it.

My father's mind was not of that order whose natural walk is original invention, yet he nevertheless, occasionally, assumed the character of a projector. I would notice his ingenious suggestion of the new form of Suspension Bridge, applicable to small spans, by which the necessity for tall piers is avoided, and which has been partially adopted in the bridge over the Thames at Hammersmith. In designing a timber bridge for the Meikle Ferry, he proposed a new form of arch of a beautiful and simple construction, in which what might be called the ring-courses of the arch, are formed of layers of thin planks bent into the circular form and stiffened by *king-post-pieces*, on which the level roadway rests. This form of bridge has come into very general use on railways.

In addition to his professional exertions, he took an active part in advancing the interests of science, in so far as lay in his power; and was one of the original promoters of the Astronomical Institution, out of which has grown the present establishment of the Royal Observatory. In 1815 he became a Fellow of the Royal Society of Edinburgh; and he afterwards joined the Geological Society of London, and the Wernerian and Antiquarian Societies of Scotland. He also made several contributions to the Encyclopædia Britannica, to Brewster's Edinburgh Encyclopedia, and to various scientific Journals of the day; and gave (in a series of Letters which appeared in the Scots Magazine in 1817), a lively and instructive account of a tour through the Netherlands, in which he described some of the most interesting engineering works connected with the drainage and embankment of Holland.

I have already said, that Mr Stevenson possessed great sagacity, fortitude, and perseverance; and these were the chief points of his character. In private life he was a man of sterling worth; and, whether we regard him as a husband, a father, or a friend, he was equally distinguished by the absence of selfishness and by his great generosity. His exertions in forwarding the progress of young men through life were extensive and unwearied; and few men had more solid grounds than he for indulging in the pleasing reflection that, both in his public and private capacity, he had consecrated to beneficial ends every talent committed to his trust.

He was a man of sincere and unobtrusive piety; and although warmly attached to the Established Church of Scotland, of which for nearly forty years he had been an elder, he had no taint of bigotry or of party feeling. All his life he had been a friend of order; and in those troubled times which followed the first burst of revolutionary madness in France, he cheerfully and zealously gave up a portion of that time, which constituted his only inheritance, to the service of his country in the suppression of disorder, and enrolled himself as a volunteer. A high sense of duty pervaded his whole life; and he died calmly in that blessed hope and peace which only an indwelling and personal belief in the merits of a Redeemer can impart to any son of our guilty race.

At a Statutory General Meeting of the Board of Northern Lighthouses, which occurred on the 13th July last, the day after his death, the Commissioners recorded their respect for his talents and virtues in the following Minute:—

"The Secretary having intimated that Mr Robert Stevenson, the late Engineer to the Board, died yesterday morning,

"The Board, before proceeding to business, desire to record their regret at the death of this zealous, faithful, and able officer, to whom is due the honour of conceiving and executing the great work of the Bell Rock Lighthouse, whose services were gratefully acknowledged on his retirement from active duty, and will be long remembered by the Board; and to express their sympathy with his family on the loss of one who was most estimable and exemplary in all the relations of social and domestic life. The Board direct that a copy of this resolution be transmitted to Mr Stevenson's family, and communicated to each Commissioner, to the different lightkeepers, and the other officers of the Board."

On the Connection between the Colour of Substances and their Magnetic Properties. By RICHARD ADIE, Esq., Liverpool. Communicated by the Author.

The opinion that magnetism is a universal force, has of late years rapidly gained ground, chiefly, I believe, through the discovery of diamagnetism by Dr Faraday, which has called much attention to the subject, both in this country and on the continent. It was while examining a number of bodies on the torsion balance before a steel magnet, that the idea of attempting to trace a connection between the colours of bodies and their magnetism first struck me. The experiments were being made with a view to endeavour to ascertain by them the relation diamagnetism bore to magnetism, when the evidence of the colour of a body being an index to some forces which control its magnetism was so often repeated, that I determined to examine this question, and it is the result of these enquiries I now have to detail in the following pages.

The torsion balance employed had a beam of three inches long, suspended in a wooden case with a glass roof. The substances examined were placed on either end of the beam, and their magnetism there tested by approaching near to the point of rest of the balance, a steel magnet weighing two ounces. Such an apparatus forms a very delicate test for magnetism, in proof of which the operator, after some experience, can repeat over and over again experiments with bodies of the most feeble magnetic properties, and obtain by them uniform results. The torsion balance takes cognisance of degrees of magnetism, long before a magnet will shew any attraction when applied in the usual way; but, with a body strongly attracted on the torsion balance, there is another very palpable proof of magnetism. The substance to be tested must be in a pulverized form, when it is spread over a smooth sheet of paper; if strongly magnetic on the torsion balance, an ordinary steel magnet moved to and fro close underneath the paper without touching it, will set some of the particles in motion; and in several instances the powder can be streaked, occasioned by the finer particles being little disturbed, while those of a certain size follow for a time the magnet, and thus produce this appearance. I at first expected that the test of motion on paper would belong to a comparatively small number of bodies attracted by the magnet, but, since by varying the methods of preparation, I have succeeded in obtaining magnetic compounds from bodies I had no hope of doing so, the number appears to admit of a considerable increase. Bismuth, zinc, antimony, copper, all furnish compounds to move on paper to a magnet, and yet these metals are the most remarkable for their diamagnetic or non-magnetic properties. Among bodies repelled by the magnet, I have met no instance of a sufficiently intense force which would move the substance on paper by the kind of magnet described, so that this test must be understood to apply only to bodies attracted by the magnet.

Iron.—The compounds of this metal form a series that afford evidence of the existence of a connection between the colour and the magnetic properties of bodies. In the colour of the solid metal itself there is nothing to distinguish it from many metals of feeble magnetic powers, but in the finely subdivided pyrophoric state it has no equal for a black among the elementaries, save the non-metallic substance carbon. The fluorides of iron are pale-coloured powders possessed of feeble magnetic attraction, insufficient to make their particles move on paper; when heated with borax, they pass to a dark brown substance, containing oxides and fluorides of iron, which are strongly magnetic. The lactate of iron is a pale-coloured feebly-magnetic body; after ignition, the dark-coloured mixed oxides are left, which are well known for their magnetism.

The ter-sulphate of iron, and the alums formed with it, furnish three pure light-coloured bodies, quite magnetic on the torsion balance, but not enough so to move when pulverised on paper; by heat the dark oxides are produced, that move readily to the magnet. But it is to the ferro-cyanide of potassium that I wish to refer for the best proof we have of a connection between the colour and the magnetic properties. If it were a solitary case the change would at once be set down to the decomposition of the cyanogen and the formation of a carburet of iron. In the sequel it will be seen that facts of a similar kind occur under such varied circumstances, that this explanation will not suffice. The ferro-cyanide of potassium is a translucent lemon-yellow salt, possessed of no attraction for the magnet; when heated moderately, it loses water and assumes an opaque white hue, but is still unattracted by the magnet; when the heating is continued till the colour darkens, then the degree of darkness becomes an index of the magnetic force, until the colour reaches black, when the altered salt has all the characters of an iron body.

Nickel.—The salts of this metal are generally coloured, and the magnetic properties of their base are seen reduced in a greater or less degree in them. The oxalate of nickel is a pure pale-coloured body, magnetic only on the torsion balance; when heated to redness it passes to a dark colour, and the new compound moves readily on paper to a magnet underneath.

Manganese.—Proved by Berthier to be a magnetic metal, which an examination of its salts on the torsion balance tends strongly to confirm, for there they resemble those of iron and nickel; the colourless sulphate of manganese is as much attracted by the magnet as the transparent iron alum, but both are inferior to several dark-coloured compounds of non-magnetic metals. Carbonate of manganese is a pale-coloured substance which cannot be shewn to be magnetic without the torsion balance; by heat it is changed into a brown-tinted oxide which, spread out on paper, moves feebly to a magnet underneath. *Arsenic* in its pure state is magnetic on the torsion balance, but when pulverised is not disturbed by a magnet. In union with its equivalent of sulphur the ruby-red realgar is produced. Sulphur has been shewn by Faraday to be diamagnetic, yet in this union with arsenic a compound is given that is far more magnetic than the metal or any other of its products, for realgar when bruised and spread out on paper is moved and can be streaked by a magnet. In most of the foregoing cases given, the dark magnetic substance is an oxide formed or set free by heating; with this metal oxygen forms the well known white oxide, a diamagnetic, according to Faraday, but which, with the feebler magnetic force from a small steel magnet, proves slightly magnetic; however this may be, the contrast is striking between the feeble magnetism of the white oxide and the decided magnetism of realgar. It was this contrast that led me to try to trace the connection between colour and magnetic properties, which, on account of the near relation of the colour of iron to many other non-magnetic metals I felt a great reluctance to do.

Silver is a metal possessed of only feeble torsion balance magnetism; in its dark-coloured sulphuret and oxide the attraction is more decided; the white chloride is like the pure metal, very feeble in its attraction. Through the influence of light it passes to the dark oxide, gaining magnetic force with the change. Should these experiments be confirmed, this well known action of light on the chloride of silver will serve as a very beautiful illustration, for in it the whole question is resolved; the light effects a change in the colour; the alteration of the colour is accompanied by an increase of magnetic attraction.

Palladium, feebly magnetic on the torsion balance, furnishes a dark-coloured oxide, which, if well dried, moves to the magnet on paper, while the sulphuret, a similar body, has not sufficient magnetism to do so.

Copper.—This metal possesses a degree of magnetism so minute that it is recognised with the utmost difficulty on the torsion balance, yet it gives the dark-coloured oxide and sulphuret, both of which are strongly magnetic; for if well dried specimens are strewed on a sheet of paper, some of their particles are moved after the manner described, but not enough so as to allow the powder to be streaked like the sulphuret of arsenic. The diniodide of copper is a pale-coloured substance, more magnetic than copper, but not sufficient to move in the least degree on paper; when heated on silver it parts rapidly with iodine and darkens in hue; if the heat be continued till the iodine is nearly all exchanged for oxygen, the dark-coloured body left moves as freely, if not more so, on paper, than the pure black oxide. A result similar to this I have also obtained, where the oxide of copper had been digested in hydrofluoric acid, then heated with a flux to 600, and the insoluble dark-coloured residue washed out to be tested.

Lead and Tin.—These metals possess more magnetism on the torsion balance than many of the white-coloured substances they enter into. Their coloured oxides and sulphurets are more magnetic than the metals, but inferior to the similar compounds of the other metals already noticed. The pure white fluoride of lead is the least magnetic of the metallic fluorides; when fused, it passes to a light greyish brown, and gains in attractive force. In this powder there are a few darker-coloured particles that move readily to the magnet on paper. The colourless acetate of lead is repelled by the magnet; decomposed by heat it yields a dark-coloured carburet sufficiently magnetic to move slightly on paper to a magnet underneath. The fine yellow-coloured chromate of lead is nearly quite inert before the magnet; fused brown, it becomes nearly equal to the carburet of lead in attractive force.

Bismuth, Antimony, and Zinc, in their pure state, have been shewn by Dr Faraday to belong to the class of diamagnetic bodies; in union with oxygen or fluorine they give light-coloured substances decidedly magnetic by the torsion balance, which would indicate that these metals are magnetic to a considerable degree, but that the magnetism is masked by some force, most probably connected with their crystallization. This supposition led me to a long tedious search among the compounds of these metals for bodies that would exhibit their attraction when spread on paper. Ultimately I found that partially-reduced or sub-oxides could be prepared in a simple manner, possessed of strong attraction, and all of them darker in colour than the oxide from which they had been derived. In their preparation the colour is a guide to the success or failure of the process; for if the heat is carried too far, the reduced portion is re-oxidized, the colour is too light, and the magnetic property is lost. Bismuth is the most diamagnetic of the elementary bodies—the light-coloured oxide is attracted on the torsion balance when heated with a flux composed of equal parts of borax and citric acid crystals bruised, a mixture is washed out from the residue, or even in the unwashed residue pulverised there are particles which move with the utmost facility to a magnet. The light-coloured oxide and fluoride of zinc are magnetic only on the torsion balance ; the acetate of zinc is colourless and diamagnetic ; when ignited a mixture of oxide and sub-oxide remains, the darker particles of which are decidedly magnetic on paper. This is one of the simplest experiments for shewing a magnetic product from non-magnetic elements, the pure acetate of zinc can be so readily obtained, and only requires heating. Antimony, like zinc, has magnetic oxides and fluorides of pale hue, both of which, by fusing with borax, give darker-coloured partially-reduced oxides, that move readily on paper to a magnet. The black sulphuret also has particles of a similar description before any reduction of the sulphuret has been tried, but the effect is increased when this compound has been subjected to a partial reduction.

Potassium and Sodium.—These light-coloured metals are very nearly inert on the torsion balance : this character likewise belongs to their colourless oxides, fluorides, chlorides, and the numerous white or transparent bodies into which they enter, and their dark-coloured sulphurets are also of a similar nature. The only case I have met with to the contrary is iron alum, already alluded to, where its colourless nature is only adequate to subdue a portion of the magnetic tendency of the iron.

Carbon, in the form of a pure diamond, is feebly attracted ; in the finely-subdivided black state the attraction increases, and in the coal left after the decomposition of colourless starch or sugar, there is a great increase in the magnetic attraction, so much so, that I had hopes of being able to move this carbon on paper ; but to do so, I find that I must increase the energy of my test. The coal from sugar is known to contain oxygen and hydrogen in combination, so that this kind of carbon resembles in composition the partially-reduced products described as obtained from the diamagnetic metals, while it also closely resembles them in attraction for the magnet.

Cadmium.—On the torsion balance this metal has a very slight tendency to leave the magnet. Its carbonate is a pure snow-white substance, possessed of no magnetic power—by heat it passes to the oxide of a brick-red colour, accompanied by a rise in magnetic force, just sufficient to make a few granules of the powder when spread out move to a magnet underneath. The oxide of cadmium, ignited with citric acid,* leaves a residue containing more magnetic particles than the oxide reduced from the carbonate. In this case, and frequently in these experiments, the use of an organic acid which, during decomposition, furnishes carbon, is apparent. In the destructive distillation of the acetate of lead, Berzelius states the residue to be a carburet, a body which the experiments given have

---

* This acid was tested for iron by ferro-cyanide of potassium, of which it shewed not a trace.

shewn to contain magnetic particles. A similar decomposition of the acetate of zinc probably gives some mixture of a carburet with the oxide of the metal, which produces the high magnetic character of some of the darker particles. Again, in the three metals, mercury, silver, and tin, nearly allied to cadmium in colour, there is no evidence of motion on paper to a magnet to be obtained from their oxides or sulphurets until these have been fused, and more or less ignited with citric acid. The oxide of silver, though easily partially reduced by heat, will not, by such treatment, give the required particles—for them the black sulphuret must be ignited with citric acid when the residue contains a few that move feebly to a magnet. Oxides and sulphurets of tin and mercury likewise furnish, when heated with citric acid, particles that move to a magnet, but only very feebly, and these two metals are in every respect inferior to the three diamagnetic ones for yielding magnetic products.

Platina.—The brown bichloride of this metal has a feeble attraction for the magnet on the torsion balance; decomposing it by heat alone, I did not succeed in getting a product that would move on paper; but, as in the case of silver and mercury, by adding a vegetable acid, and reducing the mixture at a moderate heat, a dark-grey powder was left, of which some of the particles moved to a magnet.*

Gold supplies a very pretty illustration of the connection of the colour with the magnetic properties of bodies. The cyanide, a fine canary-coloured powder, is quite magnetic by the torsion balance test, but immoveable to a magnet on paper; by a gentle heat, the cyanogen is exchanged for oxygen, the powder assumes a dark-brown colour, which moves on paper to a magnet; by further heating, the oxygen is expelled, and pure gold remains.

Cobalt.—The ruby-red crystals of the sulphate of this metal have not sufficient magnetism to move on paper when spread out in a pulverised state; by heat, they are converted into the black oxide, which is more acted on by the magnet than any of the metals or their compounds described, save those of iron and nickel. This oxide, heated in a tube through which a current of hydrogen is passed, is reduced to the pyrophoric cobalt, a pure black body very nearly equal to nickel, prepared in a similar manner, in the extent of the motion of its particles before a magnet.

I may now pause to note, in the foregoing experiments, a result which they have made apparent, and which I did not anticipate at their commencement; it is, *that all the heavy metals in everyday*

---

* When the number of particles moveable to a magnet form a very small proportion of the whole body, it may eventually prove that this effect is connected with the presence of some foreign matter; but where I suspected this, I used a spirit-lamp flame to reduce the oxide, and I have not yet been able to shew the motion to be due to the presence of iron.

use in the arts have furnished either oxides, carburets, sulphurets, or fluorides, which contain particles sufficiently magnetic to move on paper to an ordinary steel magnet passed to and fro underneath.

Iron, nickel, and cobalt, are apart from all the other elementary substances for their ready motion on paper to a magnet. Manganese, pure, and arsenic in the black or supposed partially-oxidized form, approach them; but in so much lower degree, that they class better with metals that come near to them in magnetic attraction when tested on the torsion balance. The distinctive magnetic property of these three metals naturally introduces the question, in an inquiry like the present, Is there anything peculiar in their colour? To which I think it must be answered, that in their solid form there is not; for nickel ranks between silver and tin in colour, two metals of very low magnetic properties. But if the colours of the precipitates from the oxides of these metals be looked to, a difference is observable, shewing the more magnetic metal to produce a larger proportion of coloured precipitates; if five of the most important reagents be employed to throw down precipitates from oxide of nickel, four of them are coloured and one white; while the two oxides of tin precipitated by the same means, give four coloured and six white. Silver produces three coloured and two white substances from its oxide. Referring to a table of precipitates from the oxides of the heavy metals by the reagents in use for testing,* the proportion of coloured to white, taken in the aggregate, is very nearly as 2 to 1; for the magnetic metal nickel, the same substances are as 4 to 1; and for the non-magnetic metals silver and tin, as 7 to 8; thus shewing that a magnetic metal has a far stronger tendency to form coloured bodies than one which has feeble magnetic properties. Iron and cobalt, in their solid form, are not distinguished by any peculiar colour from less magnetic metals, but in their pyrophoric form they supply the purest blacks found among the metals. The proportion of their coloured precipitates to white ones, is, like nickel, above the average, which has been stated at two coloured to one white; iron and cobalt give nearly six coloured to one white; and if these metals be compared with mercury and platina, that with difficulty afford products sufficiently magnetic to move on paper, the proportions for the precipitates are 6 to 1 for the magnetic against 3 to 1 for the non or feeble magnetic metals. Or if the comparison be made with the five diamagnetic metals, antimony, bismuth, zinc, cadmium, tellurium, the differences widen, for these yield thirteen white precipitates, and only eight coloured; proving that, independently of my experiments, he data furnished from a published table of precipitates, shew the

* Published by Maclachlan and Stewart, 1832. This table gives eight reagents, but three I treat as duplicates of three others, namely, sulphuretted hydrogen and hydrosulphuret of ammonia, caustic potash and ammonia, carbonates of soda and ammonia.

magnetic metals, iron, nickel, and cobalt, to throw down a much larger proportion of coloured substances than the diamagnetic metals.

It is on these results that I rely for proof of the connection between the colour and the magnetic properties of bodies; their general tendency is to shew that, when the forces of aggregation which bind the particles of a substance together produce transparency or whiteness, such a combination has feeble magnetic properties; and when the same forces produce a dark or dull coloured substance, then the magnetic power is more developed. For the proof of this I have given the comparison among a number of bodies of like constitution, where the rule has appeared invariable; but if a similarity of constitution be disregarded, exceptions can be readily given; if transparent iron alum, sulphate of manganese, or nitrate of silver, be compared with dark-coloured sulphurets of potassium or sodium, there is then no connection to be traced between colour and magnetic properties. *The relation, then, between colour and magnetic attractions of bodies must be held to rest only among those of similar constitution.*

In the course of the experiments I have given, the tendency of oxygen, where it forms a dark-coloured oxide, to develope decided magnetic attraction, has been shewn in the case of copper and silver; where oxygen unites to form colourless combinations, the great majority of these are either diamagnetic or are feebly attracted. Dr Faraday has shewn that water and the gaseous transparent oxides are repelled by the magnet. In my experiments with the torsion balance, rock crystal, Iceland spar, boracic acid, sugar, starch, resins, all of them colourless bodies containing oxygen, are repelled by the magnet; while talc and alum or sulphate of potassa and alumina, are feebly attracted.—Atmospheric air drawn by a fine-pointed pipet from between two highly-magnetised steel surfaces, when tested for oxygen, gave the usual proportion. Now, when bodies are decidedly magnetic in themselves, this distinguishing feature can be traced in the greater part of their combinations. This is the case for the four magnetic metals, iron, cobalt, nickel, and manganese, where the ferrocyanide of potassium is the only exception. In the oxides, the diamagnetic transparent ones are so numerous that it appears to me fair to argue from them *that oxygen, like pure diamond, has only feeble magnetic properties.* The attraction of talc and alum I do not consider to militate against this view, for the aluminum contained in them has very decided magnetic properties.

From inorganic matter I turned to examine some organic bodies where colours of the most surpassing beauty are prepared in nature. The wings of British moths and butterflies were tried on the torsion balance; but these, beginning with the white cabbage butterfly, being all attracted, gave me no evidence. One specimen, the body of a moth, was repelled, evidently by a quantity of dried gelatinous matter with which it was filled.

The leaves and corollas of plants proved to be better adapted for the inquiry; still, for definite results, not nearly so much to be relied on as the chemical compounds I have already given. The lignine, or matter forming the walls of the sac, which is now known to be the skeleton of every vegetable production, is nearly inert before the magnet, but when charged with water or resin, then the tendency is to leave it; and this frequently occurs in flowers and organic bodies irrespective of their colour. For example, sap green, a precipitate by lime from a red-coloured vegetable juice, has a very dark green, nearly black colour, and a slight diamagnetic tendency. By alcohol a translucent yellow-green substance is dissolved out, which is diamagnetic, while the residue, a dark earthy-green body, is attracted by the magnet. From a number of experiments with coloured parts of plants of various shades, I find the white and pale yellow repelled, while the darker and less tranlucent colours are attracted by the magnet. Among the repelled are the corollas of the white daisy and pale yellow everlasting flower; calyx of the snowdrop when fresh, but attracted when it begins to wither; rein-deer moss, and some light-coloured green leaves in a moist state. Among the attracted are the oak and fern leaves dried brown, tutsan leaves dried, black tea, rose corolla leaves, only after the odoriferous matter was well washed out, dark green leaves of evergreens, either fresh or dried, corolla of the common gorse, wallflower, deep-coloured violets, primroses of a reddish-brown hue, polyanthus corolla dark-coloured, and some lilac and crimson leaves of flowers.

With the approach of summer I look forward to being able to increase the number of diamagnetic corollas, which probably may be got of every hue provided they have sufficient translucency, a condition the foregoing inquiry has shewn to have so much influence in producing the property of magnetic repulsion.

On the Physiognomy of the Islands in the Pacific. By Mr James D. Dana.

The valleys of the Pacific Islands have usually a course from the interior of the island towards the shores; or when the island consists of two or more distinct summits or heights (like Mani), they extend nearly radiately from the centre of each division of the island. They are of three kinds:—

I. A narrow gorge, with barely a pathway for a streamlet at bottom, the enclosing sides diverging upwards at an angle of thirty to sixty degrees. Such valleys have a rapid descent, and are bounded by declivities from one hundred to two thousand feet or more in elevation, which are covered with vegetation, though striped nearly horizontally by parallel lines of block rock. There are frequent cascades along their course; and at the head, they often abut against the sides of he central inaccessible heights of the island. The streamlet has frequently its source in one or more thready cascades that make an unbroken descent of one or two thousand feet down the precipitous yet verdant walls of the amphitheatre around.

II. A narrow gorge, having the walls vertical or nearly so, and a flat strip of land at the bottom more or less uneven, with a streamlet sporting along, first on this side and then on that, now in rapids, and now with smoother and deeper waters. The walls may be from one hundred to one thousand feet or more in height; they are richly overgrown, yet the rocks are often exposed, though everywhere more than half concealed by the green drapery.

These gorges vary in character according to their position on the island. Where they cut through the lower plains (as the dividing plain of Oahu) they are deep channels with a somewhat even character to the nearly vertical walls, and an open riband of land at bottom. The depth is from one to three hundred feet, and the breadth as many yards. Farther towards the interior, where the mountain slopes and vegetation have begun, the walls are deeply fluted or furrowed, the verdure is more varied and abundant, and cascades are numerous.

This second kind of gorge, still further towards the interior, changes in character, and becomes a gorge of the first kind, narrowing at the bottom to a torrent's course, along which are occasional precipices which only a torrent could descend.

III. Valleys of the *third* kind have an extensive plain at bottom quite unlike the strip of land just described. They sometimes abut at head against vertical walls, but oftener terminate in a wide break in the mountains.

The ridges of land which intervene between the valleys, have a flat or barely undulated surface, where these valleys intersect the lower plains or slopes; but in the mountains they are narrow at top and sometimes scarcely passable along their knife-edge summits. Some of them, as they extend inward, become more and more narrow, and terminate in a thin wall, which runs up to the central peaks; others stop short of these central peaks, and the valleys on either side consequently coalesce at their head, or are separated only by a low wall, into which the before lofty ridge had dwindled. The crest is often jagged or rises in sharp serratures.

The main valleys, which we have more particularly alluded to above, have their subordinate branches; and so the ridges in necessary correspondence, have their subordinate spurs.

As examples of the valleys and ridges here described, we introduce a brief account of an excursion in the Hanapepe Valley on Kauai, one of the Hawaiian Islands, and a second up the mountains of Tahiti.

Hanapepe Valley, Kauai.—We reached its inclosing walls, about four miles from the sea, where the sloping plain of the coast was just losing its smooth undulating surface, and changing into the broken and wooded declivities of the interior. The valley, which had been a channel through the grassy plain, a few hundred feet in depth, was becoming a narrow defile through the mountains. A strip of land lay below, between the rocky walls, covered with deep green garden-like patches of taro, through which a small stream was hastening on to the sea.

We found a place of descent, and, three hundred feet down, reached the banks of the stream, along which we pursued our course. The mountains, as we proceeded, closed rapidly upon us, and we were soon in a narrow gorge between walls one thousand feet in height, and with a mere line of sky over head. The stream dashed along by us, now on this side of the green strip of land, and then on that, occasionally compelling us to climb up and climb among the crevices of the walls to avoid its waters, where too deep or rapid to be conveniently forded. Its bed was often rocky, but there was no slope of debris at the base of the walls on either side, and in the greater part of the distance it was bordered by plantations of taro. The style of mountain architecture, observed on the island of Oahu, was exhibited in this shaded defile, on a still grander scale. The mural surfaces enclosing it had been wrought, in some places, into a series of semicircular alcoves or recesses, which extended to the distant summits overhead; more commonly, the walls were formed of a series of semicircular columns of vast size, collected together like the clustered shafts of a Gothic structure, and terminating several hundred feet above, in low conical summits. Although the sides were erect, or nearly so, there was a profuse decoration of vines and flowers, ferns and shrubbery; and, where more inclined, forests covered densely the slopes.

These peculiar architectural features proceed from the wear of rills of waters, streaming down the bold sides of the gorge; they channel the surface, leaving the intermediate parts prominent. The rock is uniformly stratified, and the layers consist of gray basalt or basaltic lava, alternating with basaltic conglomerate.

Cascades were frequently met with; at one place, a dozen were playing around us at the same time, pouring down the high walls, appearing and disappearing, at intervals, amid the foliage, some in white foamy threads, and others in parted strands imperfectly concealing the black surface of rock beneath.

A rough ramble of four miles brought us to the falls of the Hanapepe. The precipice, sweeping around with a curve, abruptly closed the defile, and all further progress was therefore intercepted. We were in an amphitheatre of surpassing grandeur, to which the long defile, with its fluted or Gothic walls, decorated with leaves and flowers and living cascades, seemed a fit porch or entrance way. The sides around were lofty, and the profuse vegetation was almost as varied in its tints of green as in its forms. On the left stood apart from the walls an inclined columnar peak or leaning tower, overhanging the valley. Its abrupt sides were bare, excepting some tufts of ferns and mosses, while the top was crowned with a clump of bushes. To complete the decorations of the place,—from a gorge on the right, in the verdant mountains above, where the basaltic rocks stood out in curved ascending columns on either side as if about to meet in a Gothic arch, a stream leaped the precipice and fell in dripping foam to the depths below; where, gathering its strength again, it went on its shaded way down the gorge.

The Mountains of Tahiti commence their slopes from the sea, or a narrow seashore plain, and gradually rise on all sides towards the central peaks, the ridges of the north and west terminating in the towering summits of Orohena and Aorai, while the eastern and southern, though reaching towards the same peaks, are partly intercepted by the valley of Papenoo. Aorai is seven thousand feet in height, and Orohena, not less than eight thousand feet.

We commenced the ascent of Mount Aorai by the ridge on the west side of the Matavai Valley, and, by the skilfulness of our guide, were generally able to keep the elevated parts of the ridge, without descending into the deep valleys which bordered our path. An occasional descent, and a climb on the opposite side of the valley were undertaken; and although the sides were nearly perpendicular, it was accomplished without much difficulty, by climbing from tree to tree, with the assistance of ropes, at times, where the mural front was otherwise impassable. By noon of the second day we had reached an elevation of five thousand feet, and stood on an area twelve feet square, the summit of an isolated crest in the ridge on which we were travelling. To the east we looked down two thousand feet, into the Matavia Valley; to the west, a thousand feet into a branch of the Papaua Valley, the slopes either way, being from sixty to eighty degrees, or within thirty degrees of perpendicular. On the side of our ascent, and beyond, on the opposite side, our peak was united with the adjoining summit by a thin ridge, reached by a steep descent of three hundred feet. This ridge was described, by our natives, as no wider at top than a man's arm, and a fog coming on they refused to attempt it that day. The next morning being clear, we pursued our course. For a hundred rods, the ridge on which we walked was two to four feet wide, and from it, we looked down on either side a thousand feet or more, of almost perpendicular descent. Beyond this the ridge continued narrow, though less dangerous, until we approached the high peak of Aorai. This peak had appeared to be conical, and equally accessible on different sides, but it proved to have but one place of approach, and that along a wall with precipices of two or three thousand feet, and seldom exceeding two feet in width at top. In one place we sat on it as on the back of a horse, for it was no wider, and pushed ourselves along till we reached a spot where its width was doubled to two feet; and numerous bushes again affording us some security, we dared to walk erect. We at last stoo perched on the summit edge, not six feet broad. The ridge continued beyond for a short distance, with the same sharp, knife-edge character, and was then broken off by the Punaavia Valley. Our height afforded a near view of Orohena; it was separated from us only by the valley of Matavai, from whose profound depths it rose with nearly erect sides. The peak has a saddle-shape, and the northern of the two points is called Pitohiti. Those summits, and the ridge which stretches from thence toward Matavai, intercept the view to the southward. In other directions, the rapid succession of gorge and ridge that characterises Tahitean scenery, was open before us. At the western foot of Aorai appeared the crown. Beyond it extended the Punaavia Valley, the only level spot in sight; and far away, in the same direction, steep ridges, rising behind one another with jagged outline, stood against the western horizon. To the north, deep valleys gorge the country, with narrow precipitous ridges between; and these melt away into ridgy hills and valleys, and finally into the palm-covered plains bordering the sea.

On our descent, we followed the western side of the Papaua Valley, along a narrow ridge, such as we have described, but two or three feet wide at top, and enclosed by precipices of not less than a thousand feet. Proceeding thus for two hours, holding to the bushes which served as a kind of balustrade, although occasionally startled by a slip of the foot on one side or the other—our path suddenly narrowed to a mere edge of naked rock, and, moreover, the ridge was inclined a little to the east, like a tottering wall. Taking the upper side of the sloping wall, and trusting our feet to the bushes while clinging to the rocks above, carefully dividing our weight, lest we should precipitate the rocks and ourselves to the depths below, we continued on till we came to an abrupt break in the ridge of twenty feet, half of which was perpendicular. By means of ropes doubled around the rocks above, we in turn let ourselves down, and soon reached again a width of three feet, where we could walk in safety. Two hours more at last brought us to slopes and ridges where we could breathe freely.

The peculiarities here described characterize all parts of the island. Towards the high peaks of the interior, the ridges which radiate from, or connect with them, become mere mountain walls with inaccessible slopes, and the valleys are from one to three thousand feet in depth. The central peaks themselves have the same wall-like character. It is thus with Orohena and Pitohiti, as well as Aorai; and owing to the sharpness of the summit edge, rather than the steepness of the ascent, Orohena is said to be quite inaccessible. Dr Pickering and Mr Couthony, in an excursion to a height of five thousand feet on this ridge, met with difficulties of the same character we have described.—*American Journal of Science and Arts,* Vol. ix., No. 25. January 1850. Page 48.

On the Cause of the Phenomena exhibited by the Geysers of Iceland. By STEVENSON MACADAM, Teacher of Chemistry, Philosophical Institution, Edinburgh. Communicated by the Author.

In a memoir published in the preceding number of this Journal, I referred to certain remarkable properties possessed by bodies when they assume the spheroidal state ; and gave my reasons for believing that the spheroidicity of matter played an important part in Volcanic Phenomena. In my present paper, which may be regarded as a continuation of the preceding one, I propose to shew that the phenomena presented by the intermittent hot springs of Iceland can be clearly and satisfactorily explained by the laws which regulate matter in the spheroidal state.

The characteristic phenomena exhibited by the Geysers, before and during an eruption, have been described by many of our distinguished men of science, and the following brief notice of the mode of action of these springs, which has been compiled from the several treatises published on the subject, will convey an adequate conception of the kind of phenomena, for which I now attempt to assign the cause.

The largest and most remarkable of these springs has been termed the Great Geyser, and from the comparative magnitude of its eruptions it has claimed the special attention of all observers. We find, therefore, that our store of knowledge regarding it is very ample, while but little is known regarding the lesser springs. The phenomena presented by the latter, however, does not differ materially from those of the former ; any dissimilarity which might be adverted to, being merely that of the magnitude, not of the kind of phenomena exhibited.

Prior to an eruption of the Great Geyser, a hollow rumbling sound is heard, quickly increasing in loudness, and accompanied by explosions resembling the distant firing of cannon ; the earth being at the same time slightly shaken. The subterranean explosions, and the tremor of the earth quickly become more violent. In an instant the atmosphere surrounding the Geyser is filled with volumes of steam, while a column of boiling water shoots like an arrow to the clouds. The height which the water attains is variously estimated from 80 to 200 feet; the variation no doubt being the result of a difference in the intensity of the eruption. Successive jets of water continue to be projected from the Geyser for some minutes, after which the pipe or tube becomes empty of water, and the eruption terminates by an immense volume of steam, being emitted with tremendous force and a thundering noise.

In endeavouring to account for the cause of the phenomena in question, I assume that there exists in connection with the Geyser, a subterranean chamber of large size, and of an oblong shape; the floor of which is of a roundish form, and at a temperature of not less than 340° F. At or near the roof there are fissures communicating with reservoirs, by which water may be allowed to flow into the caverns. The tube which passes from this cavity to the surface of the earth takes its rise from the side of the chamber, and very near the lowest part. Without entering into details, I assume this tube (as other writers on Geysers have done), to be an inverted syphon, the shorter limb of which communicates with the chamber, whilst the longer limb forms the exit or emission-tube of the Geyser. In the course of events, water finds access by the fissures into the cavity, where, from the high temperature of the matter it falls upon, it is immediately compelled to assume the spheroidal condition, its temperature while in that state being 205·7° F. The water gradually accumulates, till at last so much has entered the cavity, that the mineral floor can no longer keep the liquid in the spheroidal state, the water in consequence touches the metallic surface; its temperature is almost instantly raised to 212° F.; and large volumes of steam are generated, producing a force quite competent to press the boiling water up through the opening prepared by Nature for it. Shortly thereafter, when the whole of the water, or at least a considerable portion of it, has been discharged through the conduit, and the propelling agent has thus cleared a path for itself, the steam escapes in large volumes, with a rushing sound more or less violent. The quantity of steam generated especially at the moment of contact between the water and the hot surface, easily accounts for the sounds heard, and the force which causes the earth to tremble.*

A glance at the above enumeration of facts, regarding the manner of action of water placed in the circumstances mentioned, and which can be experimentally demonstrated, will suffice to shew the applicability of those facts to account for the cause of the phenomena exhibited by the Geysers.

Two points, brought forward by different parties, remain to be considered. The first is couched in the following terms:—"Though it cannot be denied that these springs have some communication with the volcanoes which abound in the island, yet it is a remarkable fact that they are seldom found very near them."† The above quotation contains information highly favourable to the spheroidal theory of Geysers, for although the immediate presence of volcanoes might supply us with the means of accounting for the fissures being formed in the upper part of the cavity, yet, during a volcanic eruption, the neighbouring land is generally so much rent, that it would be almost impossible to suppose that the bed of a Geyser could remain entirely unfissured, which is so essential to the realisation of this theory. The second point is, that "Henderson found that, by throwing a great quantity of large stones into the pipe at Strockr, one of the Geysers, he could bring on an eruption in a few minutes." ‡ This is a point of great importance, as it furnishes another proof that the spheroidal theory of Geysers is the correct one. For if we compel water to assume the spheroidal state, by placing it in a heated vessel, and then drop into the latter a small angular fragment of any solid substance, we find that the water, which otherwise would have been retained in the spheroidal condition, immediately wets the surface of the vessel, and begins to be vaporised.

---

* With regard to the phenomena which happens in the interval between the eruptions, the reader is referred to the last paragraph of my memoir inserted in the preceding number of this Journal, substituting the word "geyser" for "volcanic."

† Ency. Brit. 7th ed. Article Iceland. Vol. xii. p. 146.

‡ Lyell's Principles of Geology, 7th ed. p. 531.

On the Electrical Phenomena of certain Houses. By ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy in New York University.

Within the past few years, several houses in the city of New York have exhibited electrical phenomena in a very remarkable degree. For months in succession they have emitted sparks of considerable intensity, accompanied by a loud snap. A stranger on entering one of these electrical houses, in attempting to shake hands with the inmates, receives a shock which is quite noticeable and somewhat unpleasant. Ladies in attempting to kiss each other are saluted by a spark. A spark is perceived whenever the hand is brought near to the knob of a door, the gilded frame of a mirror, the gas pipes, or any metallic body, especially when this body communicates freely with the earth. In one house which I have had an opportunity to examine, a child, in taking hold of the knob of a door, received so severe a shock that it ran off in great fright. The lady of the house, in approaching the speaking-tube to give orders to the servants, received a very unpleasant shock in the mouth, and was very much annoyed by the electricity, until she learned first to touch the tube with her finger. In passing from one parlour to the other, if she chanced to step upon the brass plate which served as a slide for the folding doors, she received an unpleasant shock in the foot. When she touched her finger to the chandelier (the room was lighted with gas by a chandelier suspended from the ceiling) there appeared a brilliant spark and a snap, as in the discharge of a Leyden jar of good size. In many houses the phenomena have been so remarkable as to occasion general surprise and almost alarm.

After a careful examination of several cases of this kind, I have come to the conclusion that the electricity is excited by the friction of the shoes of the inmates upon the carpets of the house. I have proved, by direct experiment, that electricity is excited by the friction of leather upon woollen cloth. For this purpose I stood upon an insulating stool, and spreading a small piece of carpeting upon a table before me, rubbed a piece of leather vigorously upon it, and then bringing the leather near the cup of a gold-leaf electrometer, the leaves were repelled with great violence. The electricity of the leather was of the resinous kind. Electricity, therefore, must necessarily be excited whenever a person walks with a shuffling motion across a carpet; but it may be thought remarkable that the electricity should be intense enough to give a bright spark. In order to produce this effect, there must be a combination of some favourable circumstances.

1. The carpet, or at least its upper surface, must be entirely of wool, and of a close exture, in order to furnish an abundance of

electricity. So far as I have had an opportunity to judge, I infer that heavy velvet carpets answer this purpose best. Two thicknesses of Ingrian carpet answer very well. A drugget spread upon an Ingrian carpet yields a good supply of the fluid. The effect of the increased thickness is obviously to improve the insulation of the carpet.

2. The carpet must be quite dry, and also the floor of the room, so that the fluid may not be conveyed away as soon as it is excited. This will not generally be the case except in winter, and in rooms which are habitually kept quite warm. The most remarkable cases which I have heard of in New York have been of close, well-built houses, kept very warm by furnaces, and the electricity was most abundant in very cold weather. In warm weather only feeble signs of electricity are obtained.

3. The rubber, that is, the shoe, must also be dry, like the carpet, and it must be rubbed upon the carpet somewhat vigorously. By skipping once or twice across a room with a shuffling motion of the feet, a person becomes highly charged, and then upon bringing the knuckle near to any metallic body, particularly if it have good communication with the earth, a bright spark passes. In almost any room which is furnished with a woollen carpet, and is kept tolerably warm, a spark may be thus obtained in winter; but in some rooms, the insulation is so good and the carpets are so electrical, that it is impossible to walk across the floor without exciting sufficient electricity to give a spark.

It may be said that in this case there can be but very little friction between the shoe and the carpet. But it must be remembered that the rubber is applied to the carpet with considerable force, being aided by the whole weight of the body, so a slight shuffling of the feet acts with great energy.

In the London and Edinburgh Philosophical Magazine for February 1839, is given an account of a leather strap connecting the drum of a worsted mill, which gave sparks two inches in length, and charged a battery in a short time. The strap was twenty-four feet long, six inches broad and one eighth of an inch thick. It crossed in the middle between the two drums, the strap forming a figure 8. Here there was considerable friction, since the strap made one hundred revolutions in a minute.

In the American Journal of Science for July 1840, is mentioned an instance of a leather band in a cotton factory, which exhibited strong electrical excitement.

These examples shew that leather, when subjected to considerable friction, yields an abundant supply of electricity.

In the Proceedings of the American Philosophical Society for December 1840, are mentioned several cases of individuals who drew sparks of electricity from a coal stove and from a common grate. I consider it probable that in those cases the experimenter was the electrified body, and not the stove or grate. How is it possible for a grate containing burning coals to be insulated, so as to retain a charge of electricity? On the other hand, it is presumed that the experimenter was insulated by standing upon a carpet made quite dry by a winter fire.—*American Journal of Science and Arts*, Vol. x., No. 30, p. 321.

On the Principles of Classification in Zoology. By Professor L. Agassiz.

It may be said that investigations upon the structure of animals have already yielded all the information coming from this source which can serve to improve our classification of the animal kingdom.

After the great general divisions of the animal kingdom have been circumscribed in accordance with their anatomical structure, after the classes of the animal kingdom have been characterized by organic differences, it is hardly possible to expect that further investigations upon the structure of animals will afford the means of establishing correctly the natural relations of the families. For it is already seen that the amount of organic difference which exists between the different families is either too insignificant to afford a test by which to settle their pre-eminence or inferiority, or so striking as to impress us with an exaggerated idea of their difference. Many examples could be quoted to shew, that, in this respect, from the same identical facts, naturalists have arrived at very opposite conclusions. And this diversity of opinion among investigators of equal ability leads me to think that comparative anatomy has done its work in that direction, and that we must seek for another principle in order to settle, in a natural way, the respective positions of the minor divisions throughout the animal kingdom, and to set aside, once for ever, the arbitrary decisions which we are constantly tempted to introduce into our classifications, whenever we attempt to arrange all the families in natural groups. Before so much had been done to improve the natural classifications of the animal kingdom, it was hardly possible to notice how much was, on every occasion, settled by induction, and even arbitrary decision, beyond what the knowledge of facts would justify; for the brilliant results which the introduction of comparative anatomy, as the foundation of the classification of the animal kingdom, has brought to light must naturally have blinded us to the imperfections and deficiencies which constantly accompany the most important improvements in the natural arrangement of every class. Nevertheless, our confidence in the possibility of ascertaining the natural relations of all animals has been increased by the growing agreement between the different systems; and there is no philosophical observer who has not noticed this process of gradual approximation towards a greater uniformity in the view taken by different observers of the natural affinities of animals, however concealed this agreement has often been in consequence of changes of name or transposition of the order in which the objects were introduced.

The time has, however, gone by when the mere translation of family names, or of more general or minor divisions, into another language, could be presented as a new system, and the raising of a secondary division into the rank of a primary group, or the lowering of a primary division into a subordinate position, constituted an improvement in the knowledge of the natural relations of animals. Nothing short of a material addition to the information we possess respecting any group of animals, can now be considered as a real advance in zoology.

It should be further considered that our object is not merely or chiefly to ascertain the structural relations of animals, but to know all the various relations which have been established between them, and which they sustain toward the world in which they live. The knowledge of the natural embryonic development—of the order of succession in geological times,—of the geographical distribution upon the surface of our globe—and of the habits arising from the natural relations to the elements in which they live,— all these considerations are of as great importance in our zoological studies as the knowledge of the structure and functions of their organs, to which, of late, more exclusive attention has been paid.

When comparing, in former years, the characters of fossil fishes, especially with a view of ascertaining their natural relations to the living types, I was struck with the fact, that those of earlier ages presented many structural peculiarities, which occur only in the embryonic conditions of the fishes of our days, and also that the older representatives of any family rank lower in comparison to their living representatives.

This led me to infer that embryonic data might be applied with advantage to the correct appreciation of the natural relation of the various members of one and the same family, and perhaps also to the determination of the relative position of closely allied types.

Under this impression, I began to compare young animals of various families with the different types of the same family in their full-grown condition, when I was forcibly struck with the close resemblance there is between the younger stages of development of such representatives as could otherwise be recognised as ranking high in their respective families, and the lower forms belonging to the same groups. This led naturally to the conclusion, that the change which animals undergo during their growth might safely be taken as a standard to determine the natural order of succession of all the representatives of any given type within the limits in which the higher ones pass successively through transient forms which the lower ones naturally present permanently in their full-grown condition.

This principle, once ascertained, led to the result, upon more extensive investigations, that a complete knowledge of the metamorphoses of animals, from the earliest period of their embryonic development to the last change they undergo before reaching their mature condition, would afford, throughout the animal kingdom, a true measure by which to ascertain precisely, and without arbitrary decision on our own part, the natural relative position of all the minor groups of the animal kingdom.

Beginning the revision of the animal kingdom with the type of the Articulata, it was not difficult, with these views, to ascertain that the worms, as a natural type, rank lowest in this department, as they represent permanently a structural adaptation which is closely analogous to the earliest condition of development of the insects : that the Crustacea constitute a class intermediate between the worms and insects, and not superior to the insects, as some naturalists would have them ; inasmuch as the highest combination of their rings presents us with an arrangement similar to that of the pupa of insects, in which the joints of the head and of the chest are combined in an immovable shield, as in the pupa of insects, and in which the joints of the abdomen alone remain movable, is also the case among the highest Crustacea. The position of the insects as the highest class, can no longer be denied, when we consider that in them the body is at last divided into three distinct regions,—head, chest, and abdomen—and that the locomotive appendages, which, in the lower classes, are so numerous and uniform along the whole length of the body, are reduced to the region of the chest, and assume there a particular development.

Again, the tranformation of the respiratory organs is an additional evidence in favour of such an arrangement, as will be admitted from the fact that worms and Crustacea have chiefly a bronchial respiration, while in insects it becomes aërial, at least in their perfect condition.

Once upon this tract, it was easy to follow out the minor changes which these animals undergo during their final transformation, and to derive from the knowledge of these changes sufficient information to assign a definite position to all the subordinate groups in each of these classes. Taking the insects for instance, into special consideration, we ascertain readily that chewing insects rank below the sucking tribes, as their larvæ are chewing worms, provided with powerful jaws, even in the case of those which, like Lepidoptera, have the most perfectly developed sucking apparatus in their mature condition.

Again, an investigation of the changes which the wings undergo in their formation, and the manner in which they are unfolded, when the perfect insect is hatched, led to the discovery that Coleopterous insects, far from ranking high, must be considered as lowest among insects, inasmuch as the upper larval wings of Lepidoptera are a sort of elytra, which, after being cast in the last moulting, are succeeded by the more perfect membranous wing, which in its turn undergoes such a development as to assign to those Lepidoptera, which have their wings folded backwards and enclosing the body, a position below those in which the wings spread sideways ; and the highest position to those which raise their wings upwards. So that these investigations have settled even the relative position of the secondary minor groups in each of these orders, and though, as yet, imperfectly traced out, they have at least shewn the principle upon which a natural classification of these animals might be carried into the most minute details, without ever leaving any point to arbitrary decision. Similar results have already been arrived at in other classes ; as, for instance, among Medusæ, where naked-eyed Discophori, with alternate generations, must be considered as the lowest type, recalling, in one of their conditions, the appearance of the inferior class of Polypi ; when the covered-eyed Discophori, with their strobiloid generation, begins in its lowest state with a medusoid polyp.

Similar facts are known among Echinoderms, in which, among Crinoids, the highest free forms begin with germs provided with a stem, thus assigning, on embryological grounds, a lower position to all those which are provided with a stem.

In the same manner has it been possible to determine the position of Bryozoa among Mollusca below Ascidiæ, upon the ground that their embryonic development is similar. It has been possible, in the same way, to assign to Pteropoda a position inferior to that of Gasteropoda proper, and not intermediate between Gasteropoda and Cephalopoda, as anatomical investigations would seem to indicate. For it is now plain that the spreading appendages of the body of Pteropoda are not analogous to the long tentacles which encircle the head in cuttle-fishes, but correspond to the vibratory rudders of the embryo in marine Gasteropoda.

Again, the position of Foraminiferæ seems to me no longer doubtful. They are neither microscopic Cephalopoda nor Polypi, as of late it has been generally thought best to consider them, but constitute a truly embryonic type in the great division of Gasteropoda, exemplifying in this natural division, in a permanent condition, the embryonic state of development of common Gasteropoda, during which the bulk of the yolk passes through the process of repeated divisions.

This principle—of embryological changes as a foundation of the natural classification in the internal arrangement of all the minor groups in the natural classes of the animal kingdom—applies with equal success to the Vertebrata.

We need only contrast the successive changes of tailless Batrachians during their metamorphoses, with the permanent forms of the caudate and branchiate types in that order, to be satisfied that the relative rank of all these genera can in no way be better determined, than by a direct comparison of the permanent forms of the whole group, with the successive changes in the embryonic condition of its higher types; and a comparison of the metamorphoses themselves, in the different genera, will leave no doubt as to which of them the highest rank should be assigned.

I have already, on other occasions, alluded to the improvements which are likely to be introduced into our classification of birds, upon considerations derived from embryological data. I may be permitted here to add, that even the classification of Mammalia will receive decided improvements upon the consideration of embryological changes. A single instance, even now, will at least shew that the true relative rank of their families can be determined in that way. We need only compare, among true Carnivora, the Plantigrades, the Digitigrades, and the web-footed Seals, with the transformation of the limbs in the embryo of cats and dogs, to be satisfied that the order in which these animals, as arranged by Cuvier, does not agree with their natural metamorphoses, and that the Plantigrades should rank below the Digitigrades, nearer to the Seals, and the Digitigrades highest; and the affinity of the Ice Bear to the Seals will further sustain this statement.

These remarks will, at the same time, shew that no investigations are at present more needed to improve our natural methods in classification, than a thorough study of young animals; and that an extensive illustration of the young of all the principal representatives of the great natural groups in the animal kingdom, would, in the present, contribute more to the advance of zoology than any amount of description of new species.

But these investigations of young animals should be made with a full knowledge of their various relations, and with the view of ascertaining chiefly these zoological peculiarities, which may illustrate more fully the value of all these relations.

There is another field of investigation, hardly yet entered upon, which is likely to contribute largely to the improvement of our classification. I refer to the study of fossils, compared in their structural peculiarities with the embryos of their living representatives. It has already been shown that many fossils of the earliest geological periods have a close resemblance to embryonic forms of the present day; and that, in their respective families, these fossils rank among the lower types.

This result, in itself, should be a sufficient inducement to trace this double relation, and to ascertain, from as many fossils as possible, whenever they are sufficiently well preserved to allow of such comparisons, what is the extent of their analogy to embryonic forms of the present period, and also what is the amount of affinity they have to the lower types of their respective classes.

I would mention, in this connection, the necessity of a revised comparison of the Trilobites, with the earliest stages of development of Crustacea, when it will be found, as I have already seen it, that almost all the genera of Trilobites seem to be the prophetic images, in a gigantic form, of the different types the Crustacea present in their embryonic state. The different degrees of development of their different types, when contrasted with each other, will go far to assign to each genus, its appropriate rank. I venture even to say, that the time will come when the relative age of fossils, within certain limits, will be as satisfactory a guide in assigning them their normal position in a natural system, as the facts derived from the study of their structure,—so intimate are the connections existing between all parts of the wonderful plan displayed in creation.

Little or no advantage has as yet been derived from the study of the relations of animals with the elements in which they live, in ascertaining their natural relations among themselves; but even in this respect we may derive valuable hints from a careful study of the geographical distribution of all animals; and the mere nature of the elements in which they live naturally.

On reviewing lately the whole animal kingdom, with a view to ascertain what is the value of the natural connection between the animals and the media in which they live, with reference to organic gradation, I have satisfied myself that aquatic types are decidedly inferior to the terrestrial; the marine inferior to the lacustrine and fluviatile ones; that those which live upon the main-land and burrow under ground are inferior to those which live above ground; that nocturnal are inferior to diurnal types; and that, under otherwise similar circumstances, representatives of one and the same group which differ in these respects, have a higher and lower rank, in accordance with their external circumstances; so much so, that where we have no other guides, an inference respecting their natural position may be fairly derived from their conditions of life.

It will thus be obvious, that as soon as we introduce simultaneously into our classification considerations derived from all these different sources; as soon as we allow the embryonic development, geological succession, geographical distribution, and relation to the natural elements, to assist us in our efforts to assign to all animals a natural position in one great system, we shall be able to sketch a far more complete picture of the great diversity which exists in nature, than if we allow ourselves to be guided chiefly by anatomical data; and my object at present is mainly to urge the necessity of studies in these different directions, with a view of improving our classification, and to insist upon the necessity of keeping in view, at the same time, all these facts, whenever we attempt to form a correct idea of the manifested relations which exist throughout the creation, as to all their different types, from the earliest period of the existence of animals up to the present day.—*Proceedings of the American Association for the Advancement of Science, held at Charleston, South Carolina, March* 1850.

On the Remains of Man, and Works of Art imbedded in Rocks and Strata, as illustrative of the connexion between Archæology and Geology. By GIDEON ALGERNON MANTELL, Esq., LL.D., F.R.S., *Vice-President of the Sussex Archæological Society, &c.*

(Read at the Oxford Meeting of the Archæological Institute, June 21, 1850).*

The beautiful, though quaintly expressed, idea of Sir Thomas Browne, that "*Time conferreth a dignity upon the most trifling thing that resisteth his power,*" is suggestive of the connection existing between Archæology and Geology; for as the antiquary, from a fragment of pottery, or a mutilated statue, or a defaced coin,—objects intrinsically valueless, but hallowed by the lapse of ages,—is enabled to determine the degree of civilization attained by a people whose origin and early history are lost in remote antiquity; so the geologist, from the examination of a pebble, or a bone, or a shell, may ascertain the condition of our planet, and the nature of its inhabitants, in periods long antecedent to all human history or tradition. And as the archæologist is often perplexed in his endeavours to decypher an ancient manuscript, from the original characters having been partially obliterated by later superscriptions; in like manner the geologist is frequently embarrassed while attempting to interpret the natural records of the physical history of the globe, from the obscurity occasioned by the successive mutations which the surface of the earth has undergone.

The investigation of the past is alike the object of both; but the antiquary limits his inquiries to the remains of man and his works, for the purpose of tracing the development of the human mind, in the various phases of society, from the dawn of civilization, and through the historic ages, down to the present time: his speculations, therefore, comprise but a comparatively brief period—the few thousand years that have elapsed since the creation of man and the animals

* A copy of this interesting Memoir was sent to us by the intelligent Author, and is here inserted in a nearly complete form.

which are his contemporaries. The geologist on the other hand, directs his views to the character and causes of the changes, that have taken place throughout the organic and inorganic kingdoms of nature; from the period when "the earth was without form and void," through the innumerable ages chronicled by the relics of the races of animals and plants which have successively appeared, and flourished awhile, and become extinct: his investigations also embrace the consideration of the physical revolutions which have swept over the earth's surface during the human epoch, and of those that are still in progress.

In the ancient sedimentary rocks, the remains of the animals and plants which inhabited the land, the rivers, and the seas, when those strata were deposited, occur in such abundance and variety, that the naturalist can readily determine the characters of the terrestrial and marine faunas and floras which prevailed in those remote eras. The elementary principles of geology are now so generally disseminated, that I take it for granted every intelligent person is aware that all the rocks and strata composing the dry land were originally in a softened or fluid state, either from the effects of water or from exposure to a high temperature;—that the strata are accumulations of mud, sand, or other detritus, the sedimentary deposits of streams, rivers, and seas, combined with the durable remains of animals and plants which lived either on the land or in the water;—that these beds of organic and inorganic materials have been consolidated by chemical and mechanical agency, and subsequently been elevated from beneath the waters, at various periods, by those physical forces which are constantly in action, in the profound depths of the earth, and of which the earthquake and the volcano are the paroxysmal effects;—and that such transmutations of the sea and of the land are perpetually taking place.

Throughout the entire series of the secondary and tertiary formations, though the most recent of the latter contain relics of species now existing, no traces of the human race have been discovered. It is only in the deltas, estuaries, and alluvial and turbary deposits, of comparatively modern times,—

in the detritus accumulating in the beds of the present seas, —in the recent tracts of limestone forming on the sea-shores, —and beneath the cooled lava currents erupted from volcanoes still in action,—that the remains of man and works of art have hitherto been found imbedded.

The contrast presented by the contents of modern deposits with those of the earlier formations, is thus eloquently enunciated by Sir Humphrey Davy in his interesting work, "The last Days of a Philosopher:"—"Were the consolidated depositions of sand and mud, now forming in the depths of the ocean, to be elevated above the waters and become dry land, how entirely different would they be in their characters from any that have preceded them! Their chief features would be the works of man—hewn stones, and statues of bronze and marble, and instruments of iron; and human remains would be more common than those of animals on the greatest part of the surface. The columns of Pæstum or of Agrigentum, and the bridges of iron and granite of the Thames, would offer a striking contrast to the bones of the crocodiles and colossal saurians, in the older rocks; or even to those of the mammoth or elephant in the diluvial strata. And whoever reflects on this subject, must be convinced that the present order of things, and the comparatively recent existence of man as the master of the globe, are as certain as the destruction of a different order, and the extinction of numerous animal forms, of which no living types now remain on the surface of our planet."

It is these modern deposits that constitute the fields of research which the antiquary and the geologist may explore with mutual advantage; for they abound in objects of the highest importance, relating to the interesting problem as to the contemporaneous existence of the human race, and certain species and genera of animals now only known by their fossil remains.

The idea that a concise view of the present state of our knowledge as to the occurrence of the relics of man and works of art in the mineral kingdom, might be acceptable to this learned society, first suggested itself to my mind from a perusal of the treatise of M. Boucher de Perthes, entitled, "Antiquités Celtiques et Antédiluviennes;" in which the author has deteriorated the value of his antiquarian labours by vague and erroneous conclusions, which but a slight acquaintance with the elements of geology would have enabled him to avoid; for the mineralogist will perceive at a glance that the so-called antediluvian works of art, figured and described by M. Boucher de Perthes, are nothing more than accidental forms of pebbles and stones, similar to those that occur in strata of immense antiquity, and which can never have been fashioned by the hand of man.

In this essay I propose to consider,—

Firstly,—The conditions under which the relics of man and his works may become imbedded and preserved in the strata now in progress of formation;

Secondly,—The occurrence of human bones, and instruments, and coins, in deposits of modern date;

Thirdly,—The presence of similar remains in more ancient sediments, associated with those of extinct animals; and

Lastly,—The probability of discovering indications of the existence of the human race in the earlier tertiary formations.

I.—*On the Imbedding of Human Remains in the Strata now in Progress of Formation.*

Notwithstanding the feeling of respect for the remains of the dead which appears to have prevailed in all ages, and that has given rise to the various modes of interment adopted by different nations from the earliest periods, and thus consigned the countless skeletons of successive generations to the grave, and mingled their dust with the superficial soil,—yet, incalculable numbers of human remains must have been at all times engulfed in the beds of lakes, and rivers, and seas, by ordinary casualties. And as the bones of man differ in no respect in their structure and chemical composition from those of mammalia, they must undergo the same changes when subjected to like physical conditions; hence the skeletons of men and animals deposited in the same stratum will be found in a similar state of mineralization. Fossil human bones, therefore, may occur in an earthy or a porous state, like those of mammalia imbedded in loose sand or earth; or of a dark brown colour, from an impregnation of iron, and retaining a large proportion of animal matter, as are those of the moa, Irish elk, and mastodon, found in morasses and turbary deposits; or they may be permeated by carbonate of lime and have the medullary cavities lined with spar, like the bones of Carnivora found beneath the stalactitic floors of caverns; or petrified by solutions of iron or other minerals, as are the remains of the extinct quadrupeds in many of the tertiary limestones, and those of the colossal reptiles in the Wealden deposits. They may also be invested with stalactite if buried in fissures or caves of limestone; or with travertine if exposed to the action of streams highly charged with carbonate of lime, like the so-called petrifying springs of Derbyshire; or impacted in ferruginous conglomerate, if deposited with implements of iron, or in a soil charged with chalybeate waters; and these effects may be produced in the course of a very brief period;—a few years, or even months, will often suffice for the formation of a compact, durable mass, in which bones, pottery, and coins, and other substances may be imbedded.

Although instances of such productions must be familiar to every antiquary, it may be instructive to notice a few examples that have come under my own observation, because they serve to illustrate the nature and origin of certain specimens, which have been regarded by authors of deserved celebrity as genuine petrifactions, of immense antiquity. Thus the eminent mineralogist, Kirwan, quotes from Schneider's "Topog. Min."—"that one hundred and twenty-six silver coins were found enclosed in flints at Grinoe, in Denmark, and an iron nail in a flint at Potsdam."* The first edition of Mr Bakewell's Introduction to Geology,† contains the following circumstantial narrative by Mr Knight Spencer. "In 1791, two hundred yards north of the ramparts of Hamburgh, in a sandy soil, M. Liesky, of that city, picked up a flint, and knocking it against another, broke it in two; in the centre of the fracture he observed an ancient brass pin; and on picking up the other half, he found the corres-

* Phillips's Mineralogy, 2d edit., Article *Flint*, p. 12.

† Published in 1813, p. 338.

ponding mould of the pin so laid bare. He presented them to Thomas Blacker, Esq., in whose possession they now are, and who has shown them to the writer of this letter." In the "Gentleman's Magazine," and other periodicals, there are notices of similar discoveries of keys, nails, coins, &c., in flints and blocks of solid stone.

During my early attempts to investigate the geological structure of the South-East of England, I one day received a note from a South-Down farmer, informing me of the discovery of a large iron nail in the centre of a flint which he had accidentally broken. I immediately rode a distance of some twenty miles to inspect this "wonderful curiosity," and was not a little surprised to find my correspondent's statement apparently borne out; for he placed in my hands a large rolled stone, closely resembling externally the usual flint boulders of the ploughed lands of chalk districts, and which had been split down the middle; on one side was imbedded a large iron nail, and deeply impressed on the opposite surface the corresponding mould. A slight inspection detected the nature of this specimen: it was not a flint, but an aggregation of fine silicious sand that had been converted into compact sandstone by a solution of iron derived from the nail, which had served as a nucleus to the sand that had gradually accumulated around it. The facts described by Kirwan and Knight doubtless admit of the same explanation; the narrators having mistaken a sandstone of modern formation for a genuine flint nodule. When residing at Brighton, I obtained many specimens of recent ironstone from the fishermen, who dredged them up from the British Channel. Cannon-balls, horse-shoes, nails, chains, fragments of bolts, bars, anchors, &c., formed the nuclei of these masses; some of which were exceedingly interesting from the variety of shells, zoophytes, and other marine productions promiscuously impacted in the same block of stone.*

Of the rapidity with which the aggregation and consolidation of loose materials take place at the bottom of the sea,

* The cement of the shell conglomerate now rapidly forming in the bed of the sea off Brighton is also ferruginous. See Medals of Creation, vol. i., p. 374.

a striking proof was afforded in Capt. Dickenson's gallant and successful operations, by means of a diving-bell, to recover the treasures of a richly-laden vessel,—the "Thetis,"—which was wrecked and sunk, in twenty fathoms water, off Cape Frio, to the east of Rio de Janeiro, in a bay bounded by granite cliffs. The floor of the ocean-bed was found to be composed of micaceous and quartzose sand, consolidated into what may be termed regenerated granite; the superincumbent pressure of the water, aided by the huge materials of the wreck of the frigate, and enormous blocks of granite, which, under the influence of the swell, acted with tremendous momentum, like the steam-hammers of a foundry, in a few weeks compressed the sand, wood, and iron, and the gold and silver coins, into solid masses of rock, which were broken up with difficulty to extract the impacted dollars.

It is unnecessary to adduce other examples of the nature and extent of the deposits which are in progress at the bottom of the present seas; but in passing to the next topic, I would solicit particular attention to the fact, that vast subaqueous accumulations of the relics of man and his works, must have been going on for ages, and imparting a character to the strata of the human epoch, of which no traces whatever are observable in the ancient formations.

II.—*On the Occurrence of the Remains of Man and Works of Art in modern superficial Deposits.*

From the phenomena thus briefly considered, the archæologist will be prepared to meet with the remains of man and his works in deposits which, though but of a recent origin in a geological sense, are of immense antiquity in relation to human history and tradition; suggesting the interesting question as to the remoteness of the period to which our present retrospective knowledge of the existence of mankind extends. In this division of the subject, my observations will be restricted to a few illustrations from the historic period.

Coins.—Coins, from their durability, and the facility with which the accomplished numismatist can determine their date, even when the inscription is obliterated, are the most instructive relics of human art that occur in the mineral kingdom. In the conglomerates accumulated in the beds of streams, lakes, and rivers, and in the masses of ferruginous sandstone dredged up from the sea, coins are not unfrequently enclosed. From the blocks of regenerated granitic stone formed around the sunk treasures of the Thetis, previously mentioned, many thousand dollars were extracted.

The following instance of the preservation of coins in a fluviatile conglomerate, the date of which can be precisely determined, is one of the most interesting examples of this kind with which I am acquainted. In the year 1831, some workmen employed in deepening the river Dove, where it winds round the base of the rock on which stand the mouldering ruins of the once regal castle of Tutbury, and forms the boundary-line that separates Staffordshire from Derbyshire, they observed, among the loose gravel spread over the bed of the stream, many small silver coins; and continuing their labours, discovered, at the depth of ten feet, large masses of a very hard ferruginous conglomerate, which, on being broken, were found to be studded with hundreds of similar pieces of money. On the discovery becoming known in the neighbourhood, scores of peasants hastened to the river, and at one time not less than three hundred persons were engaged in searching for the treasures. But those who were successful had great difficulty in detaching the coins from the stone in which they were impacted; for the money having lain for upwards of five centuries in the bed of the river, the water had gradually deposited successive layers of sand and gravel, till the heterogeneous mass was converted into a compact rock, of which the coins constituted an integral part.

The coins collected amounted to many thousands. They comprised sterlings of the Empire, Brabant, Lorraine, and Hainault; and the Scotch money of Alexander III., John Baliol, and Robert Bruce; and a complete English series of Edward I. There were likewise examples of all the prelatical coins of Edward I. and II., and of the first and second coinage of Henry III., and of the most early of Edward II. "On the whole," says a contemporary writer, "a finer museum of early English, Scotch, and Irish coins was never before, under any circumstance, opened to the inspection of the antiquary."

The nature of this numismatic conglomerate is seen in a small specimen, which I have fortunately rescued from destruction. It contains two silver coins of Edward I., so exposed as to show part of the effigy and superscription of the obverse.

The history of this accumulation of money, and consequently the age of the conglomerate, is clearly made out. In the reign of Edward II. (A.D. 1322), the forces of the Earl of Lancaster, then in open rebellion, being compelled to retreat from the royal army, crossed the Dove, which at that time was scarcely fordable, and in the haste and panic that prevailed, the military oak-chest, banded with iron, was sunk in the river. On the decay of the wooden chest, the coins it contained became intermingled with the gravel and sand; and the iron bands decomposing supplied the cement by which the loose materials were converted into a ferruginous breccia, as hard and durable as the ancient conglomerates which contain the teeth and bones of species of animals that have long since been obliterated from the face of the earth.

In the Thames, beneath the superficial mud and silt, a layer of breccia or conglomerate, in which Roman coins and pottery are imbedded, is spread over many parts of the river channel. This concrete is composed of pebbles, sand, and mud, consolidated by ferruginous infiltration. In this example, for which I am indebted to the liberality of Mr Roach Smith, there are exposed the half of a denarius of *Severus* or *Caracalla*, and a small brass of *Tetricus*. I have also specimens containing coins of the Lower Empire, that were collected from the bed of the Thames by Henry Brandreth, Esq., in whose possession I saw gold and silver Roman coins in a mass of conglomerate, dredged up many years since near London Bridge.

Skulls and other parts of the skeletons of domestic animals, as the dog, cat, sheep, have been found in this modern fluviatile deposit, in the same mineralized state as fossil bones in tertiary strata of a similar character.

The beds of all the rivers flowing through the large cities of Europe must contain deposits of this nature, and abound in the remains of man and his works. A Roman skull, thickly invested with travertine, that was dredged up from the Tiber some years since, and is now in the British Museum, is an earnest of the relics which lie buried beneath the yellow waters of that celebrated river. Were the bed of the Tiber effectually explored, there can be no doubt that layers of crystalline limestone and conglomerate abounding in objects of deep interest to the archæologist as works of art, and to the geologist from the physical conditions under which they have been preserved, would be brought to light.

Pottery.—The remains of earthen vessels are even more durable than coins; and fragments of ancient pottery occur, not only mixed with other relics in deposits, but in some places on the shores of the Mediterranean, as the chief constituents of calcareous limestone disposed in regular layers, the artificial materials having been cemented together by an infiltration of travertine. Urns, vases, &c., buried in calcareous or argillaceous strata, are often incrusted with tufa, or studded with crystals of carbonate or sulphate of lime, as on a Roman lamp, which was dug up near Naples, by my friend, Sir Woodbine Parish.

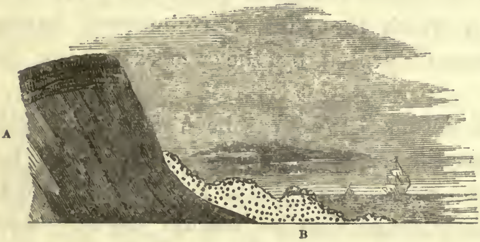
Fossil Human Skeletons.—About forty years ago, great interest was excited by the unexpected discovery of several human skeletons, male and female, in hard limestone, on the north-east coast of the Isle of Guadaloupe; and a specimen found on board a French vessel, captured by one of our cruizers, and presented to the British Museum, afforded English naturalists an opportunity of investigating the nature and age of this first known example of the bones of *Man* in a fossil state. An excellent memoir by the eminent mineralogist and geologist, Charles König, Esq., of the British Museum, published in the "Philosophical Transactions for 1814," fully elucidated the nature of these relics.

This specimen is placed in the British Museum, another, but more interesting specimen of a similar kind, also from Guadaloupe, is preserved in the National Museum at Paris.

In it the skeleton is in a bent position; and part of the lower jaw with teeth, together with a considerable portion of the upper and lower extremities of the left side, are preserved.

These fossil remains were extracted from a sloping bank of limestone, that extends from the base of the steep cliffs of the island to the sea-shore, and is almost wholly submerged at high tides, as shown in the annexed diagram.

This limestone is composed of consolidated sand, and the detritus of shells and corals of species that inhabit the neighbouring sea. Land-shells, fragments of pottery, stone arrow heads, carved wooden ornaments, and detached human bones, are occasionally found therein. The rock is therefore identical in its origin and composition with the calcareous and arenaceous limestones now forming on the sea-shores of many countries. As, for example, on the northern coast of Cornwall, where extensive tracts of drifted sand have been converted into sandstone by the slow infiltration of water, charged with calcareous and ferruginous matter.* In intertropical climes, where the waters of the sea are often turbid with the detritus of shells and corals, the sand-drift, thrown up on the strand, undergoes a rapid transmutation of this nature. Along the shores of the Bermudas, limestone is produced by this process of sufficient hardness and durability for the construction of buildings, ere the inclosed shells have lost their colour and polish.†



PLAN OF THE CLIFF AT GUADALOUPE.
A. Ancient Rocks. B. Modern Limestone in which the human skeletons were imbedded.

In the Isle of Ascension, which is frequented by turtles for the purpose of depositing their eggs in the loose sand, to be hatched by the heat of the sun, so rapidly does this lapidification take place, that groups of eggs are often found in

* Wonders of Geology, vol. i., p. 93. † Ibid., p. 84.

the consolidated limestone, containing the hatched remains of the chelonian reptiles that had thus been entombed alive.* This conglomerate consists of the water-worn detritus of corals and shells, with fragments of lava and scoriæ, rendered solid by infiltration of carbonate of lime.

These facts, if duly considered, will enable us to receive without surprise the result of an accurate investigation of all the circumstances relating to the fossil human skeletons of Guadaloupe; namely, that though imbedded in compact rock, and with the bones permeated by crystallized carbonate of lime, they are the relics of some individuals of a tribe of Gallibis, slaughtered by the Caribs, in a conflict that took place near the spot not more than 150 years ago; the sand of the sea-shore, in which the slain were interred, having subsequently become indurated by the process above described.†

Fossil human skeletons have also been found in solid calcareous tufa, near the river Santa, in Peru. Bones belonging to some scores of individuals were discovered in travertine, containing fragments of marine shells, which retained their original colour; yet this bed of stone is covered by a deep vegetable soil, and forms the face of a hill, crowned with brushwood and large trees.

Edifices.—The changes which are continually taking place in the relative level of the land and water from the subsidence of extensive tracts of country at one period, and their subsequent elevation, are phenomena so well known, that I need not dwell upon the subject; and I will, therefore, only remind the archæologist of the inexhaustible treasures of past ages, which must sooner or later be exposed to view, in the deposits that have been formed during the human epoch.

Nor can it be regarded as improbable, that in the beds of the present seas, the edifices and works of nations, whose history is altogether unknown to existing generations, are entombed and preserved. The exquisite stanzas of Mrs

---

* Wonders of Geology, vol. i., p. 90. † Ibid., 6th edit., vol. i., p. 87.

Hemans, on the hidden "Treasures of the Deep," are as true as they are beautiful :—

> "What wealth untold
> Far down, and shining through their stillness lies:
> They have the starry gems, the burning gold,
> Won from a thousand royal argosies.
>
> Yet more—the depths have more—their waves have roll'd
> Above the cities of a world gone by:
> Sand hath fill'd up the palaces of old,
> Sea-weed o'ergrown the halls of revelry."

In connection with this topic, I would refer to the ingulfing of buildings, and even entire cities, by the effects of earthquakes and volcanic eruptions; of which the catastrophe which overwhelmed Stabiæ, Herculaneum, and Pompeii, affords an illustration never to be forgotten; for after the lapse of nearly seventeen centuries, the city of Pompeii was disinterred from its silent tomb, in that marvellous state of conservation so graphically described by one of our most eminent living authors.* "All vivid with undimmed hues—its wall fresh is if painted yesterday—not a tint faded from the rich mosaic of its floors—in its forum the half-finished columns, as left by the workman's hands—before the trees in its gardens the sacrificial tripod—in its halls the chest of treasure—in its baths the strigil—in its theatres the counter of admission—in its saloons the furniture and the lamp—in its triclinia the fragments of the last feast—in its cubicula the perfume and the rouge of faded beauty—and everywhere the skeletons of those who once moved the springs of that minute but gorgeous machinery of luxury and of life."†

III.—*On Human Remains associated with those of extinct Animals in the ancient Alluvial Deposits.*

Although the relics of man and his works have been found in many places associated with the bones of extinct species

---

* Sir Edward Bulwer Lytton's "Last days of Pompeii."

† An extended review of all the facts relating to the submergence of cities, edifices, and tracts of country, will be found in Sir Charles Lyell's Principles of Geology.

of animals, yet the circumstances under which such collocations have occurred have generally, upon a rigid examination, failed to establish the synchronism of the human and quadrupedal remains. Assemblages of this nature have been observed in various ossiferous caverns in England, and on the Continent, and in South America. It will suffice for my present purpose to select the following instance, which has lately been communicated to the Geological Society of London, because it presents an epitome of the various facts which bear on this problem.

Every one knows that near Torquay, in Devonshire, there is a chasm or fissure in the limestone strata, named "Kent's Hole," which has long been celebrated for the quantities of fossil bones belonging to extinct species of bears, hyenas, lions, tigers, &c., that have from time to time been dug up from its recesses. These remains occur in a bed of reddish sandy loam, which covers the bottom of the chasm, or cavern, to a thickness of twenty feet. The teeth and bones are for the most part in an excellent state of preservation. The principal chasm is 600 feet in length; and there are several lateral fissures of less extent. A bed of hard, solid stalagmite, from one to four feet thick, is spread over the ossiferous loam, and covered with a thin layer of earth, with here and there patches of charcoal mixed with human bones, and coarse earthen vessels.

On breaking through the sparry floor, the red loam, containing teeth and bones, is brought to view; *and imbedded in it, and at a depth of several feet, and intermingled with remains of extinct bears and carnivora*, there have been discovered several flint knives, arrow and spear heads, and fragments of pottery. The stone implements are of the kind usually found in early British tumuli, and doubtless belong to the same period; yet here they were unquestionably collocated with fossil bones of immense antiquity, and beneath the impermeable and undisturbed floor of the cavern which was entire till broken through by the exploration that led to the exhumation of these relics. This discovery gave rise to many curious speculations, because it was supposed to present unequivocal proof that man, and the extinct carnivora, were the contemporary inhabitants of the dry land, at the period when the ossiferous loam was deposited: but the facts described do not appear to me to warrant this inference. Kent's Hole, Banwell Cave, and indeed all the ossiferous caverns I have examined, are mere fissures in limestone rocks that have been filled with drift while submerged in shallow water, and into which the limbs and carcasses of the quadrupeds were floated by currents; for the bones, though broken, are very rarely waterworn, and consequently must have been protected by the muscles and soft parts. Upon the emergence of the land, of which the raised beds of shingle afford proof, the fissures were elevated above the waters, and gradually drained; the formation of stalactites and stalagmites, from the percolation of water through the superincumbent beds of limestone, then commenced, and continued to a late period.

If, when Kent's Hole first became accessible, and while the floor was in a soft or plastic state, and before the formation of the stalactitic covering, some of the wandering British aborigines prowled into the cave, or occasionally sought shelter there, the occurrence of stone instruments, pottery, bones, &c., in the ossiferous loam, may be readily explained; for any hard or heavy substances, even if not buried, would quickly sink beneath the surface to a depth of a few feet, and afterwards become hermetically sealed up, as it were, by the crust of stalagmite that now forms the solid pavement.

Certain caves in Aquitaine contain masses and layers of a stalactitic conglomerate, composed of bones of men and carnivora, and fragments of pottery. The origin and formation of this breccia are attributed by M. Desnoyer to the remains of some of the aboriginal celtic tribes, who frequented these caves, or were buried there, having become blended with the mud, gravel, and debris of the extinct animals, already entombed; the mass, by a subsequent infiltration of stalagmite, having been converted into a solid aggregate.

From what has been advanced, the archæologist will therefore perceive that the occurrence of the remains of man with those of extinct species of animals, in a deposit that is covered by a thick layer of solid rock, must not be regarded as a certain proof that the human bones are of as high antiquity as those of the quadrupeds with which they are associated.

But another source of fallacy as to the presumed high antiquity of human skeletons found in sedimentary deposits, requires a brief comment. It not unfrequently happens that, from the subsidence of tracts of country, or the undermining of cliffs and headlands, or by the falling in of the roofs of caverns, the superficial soil is overwhelmed and buried beneath the strata on which it was originally superimposed. The contents of sepulchral mounds and the remains of domestic animals may thus be engulfed in very ancient deposits, at considerable depths beneath the present terrestrial surface. Such was the case described by Sir Charles Lyell, of part of a human skeleton found imbedded in a ravine on the banks of the Mississippi, with bones of the Mastodon.*

The following instance, mentioned by Mr Bakewell, holds out a salutary caution as to the necessity of the most scrupulous investigation of all the circumstances connected with a discovery of this nature.† "A thick bed of coal on the estate of the Earl of Moira, in Ashby Wolds, which is covered by strata of ironstone, coal, sandstone, &c., is worked at the depth of 225 yards. In an adjoining locality the same bed was reached at the depth of 97 yards; and in this stratum the skeleton of a man was found *imbedded in the solid coal*, which apparently had never been disturbed." No traces could be perceived that the spot had ever been dug into, or that any trials for coal had been made; but the noble proprietor, at Mr Bakewell's suggestion, directed passages to be cut in various directions, and at length the indications of a former shaft were discovered, though the coal had not been worked. Into this shaft the man must have fallen, and the body been pressed and imbedded in the loose rubbly coal by a superincumbent column of water, previously to the falling in of the pit.

* A Second Visit to the United States, vol. ii., p. 196.

† Bakewell's Introduction to Geology, 5th edition, p. 21.

Human remains imbedded with those of the fossil Elk of Ireland.—Of the extinct terrestrial mammalia of the British Isles, the gigantic Deer, commonly known as the fossil Irish Elk, is one of the most remarkable, from its magnitude and the abundance and excellent state of preservation of its remains. This noble animal was ten feet in height from the ground to the top of its antlers, which are palmated and measure fourteen feet from the extremity of one horn to the other. The bones of the Irish Elk occur in the beds of marl which underlie the peat-bogs, and are generally very perfect, being stained more or less deeply by tannin and iron, and sometimes partially incrusted with pale blue phosphate of iron: even the marrow occasionally remains in the state of a fatty substance, which will burn with a clear lambent flame. Groups of skeletons have been found crowded together in a small space, with the skulls elevated and the antlers thrown back upon the shoulders, as if a herd of deer had fled for shelter, or been driven into a morass and perished on the spot.*

Stone hatchets and fragments of pottery have been found with the bones of this creature, under circumstances that leave no doubt of a contemporaneous deposition. In the county of Cork, the body of a man, in good preservation, the soft parts being converted into adipocire, was exhumed from a marshy soil, beneath a peat-bog eleven feet thick: the body was enveloped in a deer-skin of such large dimensions as to lead to the conclusion that it belonged to the extinct Elk.†

A rib of this animal has been found in which there is a perforation evidently occasioned by a pointed instrument while the individual was alive; for there is an effusion of callus or new osseous substance, which could only have resulted from a foreign body having remained in the wound

* Skeletons of Mastodons have been found in the United States in like circumstances; and very recently remains of the colossal struthious birds of New Zealand, the Moa, or Dinornis, have been discovered by my eldest son, Mr Walter Mantell, in a morass under similar conditions.

† Jameson's Translation of Cuvier's Theory of the Earth.

for a considerable time; such an effect, indeed, as would be produced by the head of an arrow or a spear.*

Human bones have likewise been found associated with the remains of the extinct gigantic wingless birds (the *Moa* or *Dinornis*) of New Zealand, under circumstances that appear to leave no doubt of their having been contemporaneous;† but as the extinction of this family of colossal bipeds, like that of the Dodo, probably took place but a few centuries ago, those remains of man and works of art that are associated with the skeletons of the Irish Elk, may be regarded as by far the most ancient vestiges of the human race hitherto discovered. For although Indian arrow-heads and pottery have been dug up from the alluvial clay containing the bones of Mastodons, in the United States of North America, yet the evidence on this point is not conclusive. The same remark applies to the account of human crania having been found in the ossiferous caves of the Brazils, and with bones of the extinct gigantic Edentata of the Pampas.

IV.—*On the Probability of discovering traces of the Human Race in the ancient Tertiary Formations.*

The facts brought forward in the course of this argument, demonstrate the existence of man at that remote period when the Irish Elk, and other extinct species and genera of terrestrial mammalia, whose remains occur in the superficial alluvial deposits, inhabited the countries of Europe; and as the Irish Elk was contemporaneous with the Mastodon, Mammoth, and the Carnivora of the caverns, it seems not improbable that sooner or later human remains may be discovered coeval with the bones of those animals. The question therefore naturally arises, whether the evidence at present obtained warrants the inference that traces of man's existence

---

* A species of Ox (*Bos longifrons*) now extinct, was unquestionably an inhabitant of Britain during the Roman period, for its horns and bones have been found in several places associated with Roman remains; as at Colchester in 1849.—*Vide* Archæological Journal.

† By Mr Walter Mantell, of Wellington. See a Memoir on the Fossil Birds of New Zealand, Geological Quarterly Journal, 1840 and 1850; and Pictorial Atlas of Organic Remains, Art. Fossil Birds of New Zealand, 1850.

will be found in the far more ancient tertiary formations. And here it may be necessary to explain, that the geological term *Tertiary* comprises all the strata that have been deposited subsequently to the last secondary formation, the *Chalk*. The Tertiary systems, therefore, unite the present organic kingdoms of nature with the past; for while the most ancient, the *Eocene* deposits, contain the remains of a few secondary species, they have likewise many of genera now existing, associated with peculiar types.

But notwithstanding the occurrence of bones of living genera of animals—as the dog, fox, pig, sheep, ox, horse, &c.,* in tertiary strata, incomparably more ancient than the deposits containing the Irish Elk, yet no vestiges of man or of his works have been detected.

The proofs adduced of the remarkable characters impressed on the deposits that have been formed since the various races of mankind were distributed over the earth's surface, forbid the supposition that the absence of such vestiges can be attributable to their subsequent obliteration. While, therefore, we may reasonably expect to find fossil human remains in strata of much higher antiquity than those in which they have hitherto been observed, it does not seem probable that traces of man's existence will be met with in the most ancient tertiary formations.

It was for the express purpose of placing this fact in the most striking point of view, that, in a previous part of this discourse, I dwelt somewhat at length on the nature and organic remains of the deposits that have been accumulated during the human epoch. Notwithstanding, therefore, the occurrence in the Eocene system of existing genera and species of mammalia—even of that race which approaches nearest to man in its physical organization, the *Quadrumana*, or monkey tribes—I conceive we have no just grounds for assuming that physical evidence will be obtained, by which the existence of the human race, and consequently of the present order of things, may be traced back to that remote era; for I entirely concur in the opinion expressed by Pro-

---

* See Wonders of Geology, vol. i., p. 215.

fessor Whewell, "that the gradation in form between man and other animals, is but a slight and unimportant feature in contemplating the great subject of man's origin. Even if we had not revelation to guide us, it would be most unphilosophical to attempt to trace back the history of man, without taking into account the most remarkable facts in his nature —the facts of civilization, arts, government, speech, his traditions, his internal wants, his intellectual, moral, and religious constitution. If we will attempt such a retrospect, we must look at all these things as evidence of the origin and end of man's being; and when we do thus comprehend in one view the whole of the argument, it is impossible for us to arrive at an origin homogeneous with the present order of things. On this point the geologist may therefore be well content to close the volume of the earth's physical history, and open that divine record which has for its subject the moral and religious nature of man."

Observations on the Blind Fish of the Mammoth Cave. By Professor L. AGASSIZ. [In reply to a letter of inquiry from Dr Silliman, senior].

The blind fish of the Mammoth Cave, was for the first time described in 1842, in the Zoology of New York, by Dr Dekay, Part 3d, page 187, under the name of "Amblyopsis spelæus," and referred, with doubt, to the family of "Siluridæ," on account of the remote resemblance to my genus Cetopsis. Dr J. Wyman has published a more minute description of it, with very interesting anatomical details, in Vol. XLV. of the American Journal of Science and Arts, 1843, p. 94.

In 1844, Dr Tellkampf published a more extended description, with figures, in "Muller's Archiv," for 1844, and mentioned several other animals, found also in the cave, among which the most interesting is—a Crustacean, which he calls, "Astacus pellucidus," already mentioned but not described by Mr Thomson, President of the Natural History Society of Belfast. Both Thomson and Tellkampf speak of eyes, in this species; but they are mistaken. I have examined several specimens, and satisfied myself, that the peduncle of the eye only exists, but there are no visible facets at its extremity, as in other crawfish.

Mr Thomson mentions farther Crickets, allied to "Phalagopsis longipes," of which Tellkampf says that it occurs throughout the cave. Of Spiders, Dr Tellkampf found two eyeless, small, white species, which he calls "Phalangodes armata" and "Anthrobia monmouthia"—flies, of the genus "Anthomyia"—a minute shrimp, called by him "Triura cavernicola," and two blind beetles—"Anophthalmus Tellkampfii," of Erichson, and "Adelops hirtus;" of most of which Dr Tellkampf has published a full description and figures, in a subsequent paper, inserted in Erichson's Archiv, 1844, page 318.

The infusoria observed in the cave resemble "Monas Kolpoda," "Monas socialis," and "Bodo intestinalis"—a new Chilomonas, which he calls "Ch. emarginata," and a species, allied to "Kolpoda cucullus."

As already mentioned, Dekay has referred the blind fish, with doubt, to the family of Siluridæ. Dr Tellkampf however establishes for it a distinct family. Dr Storer, in his Synopsis of the Fishes of North America, published in 1846, in the Memoirs of the American Academy of Arts and Sciences, is also of opinion that it should constitute a distinct family, to which he gives the new name of "Hypsæidæ," p. 435. From the circumstance of its being viviparous, from the character of its scales, and from the form and structure of its head, I am inclined to consider this fish rather as an aberrant type of my family of Cyprinodonts.

You ask me to give my opinion, respecting the primitive state of the eyeless animals of the Mammoth Cave. This is one of the most important questions to settle in natural history, and I have, several years ago, proposed a plan for its investigation, which, if well conducted, would lead to as important results as any series of investigations which can be conceived, for it might settle, once for ever, the question, in what condition and where the animals now living on the earth, were first called into existence. But the investigation would involve such long and laborious researches, that I doubt whether it will ever be undertaken. It has occurred to me, that the final step would be a thorough anatomical study of the species found in the cave, with extensive comparison of allied species, found elsewhere—next, an investigation of the embryology of all of them, and when fully prepared by such researches, an attempt to raise embryos, of the species found in the cave, under various circumstances, different from those, in which they are naturally found at present.

If physical circumstances ever modified organized beings, it should be easily ascertained here. For my own part, however, I think that the blind animals of the cave would only show organs of vision during their embryonic state, in conformity with the normal development of the respective types to which they belong, and that even when placed under a moderate influence of light, incapable of injuring them, but sufficient to favour the growth of their eyes in the allied species provided with them, the young of those species peculiar to the cave would gradually grow blind, while the others would acquire perfect eyes; for I am convinced, from all I know of the geographical distribution of animals, that they were created under the circumstances in which they now live, within the limits over which they range, and with the structural peculiarities which characterise them at the present day. But this is a mere inference, and whoever would settle the question by direct experiment, might be sure to earn the everlasting gratitude of men of science. And here is a great aim for the young American naturalist who would not shrink from the idea of devoting his life to the solution of one great question.—*American Journal of Science and Arts*, vol. xi., No. 31, 2d Series, p. 127.

Velocity of Light. By Humboldt.

On the subject of the velocity of light, and the probability that it requires a certain time for its propagation, we find the earliest view expressed by Francis Bacon, in the second book of the Novum Organum. He speaks of the time required by a ray of light to traverse the immensity of space, and throws out the question whether the stars still exist which we now see sparkle. One is astonished at finding so happy a conjecture in a work whose celebrated author was so far below some of his cotemporaries in mathematical, astronomical, and physical knowledge. The velocity of the reflected solar light was measured by Römer (November 1675) by comparison of the times of occultation of Jupiter's satellites; and the velocity of the direct light of the fixed stars by Bradley's great discovery of the aberration of light (made in the autumn of 1727),—that demonstration to our senses of the earth's movement of translation in its orbit; viz. of the truth of the Copernican system. In very recent times a third method of measurement has been proposed by Arago, by the phenomena of the light of a variable star; for example, Algol in Perseus. We have to add to these astronomical methods a terrestrial measurement, which has very recently been executed with great ingenuity and success by M. Fizeau, in the neighbourhood of Paris. It recalls to recollection an attempt of Galileo's with two lanterns, which did not lead to any result.

From Römer's first observations of Jupiter's satellites, Horrebow and Du Hamel estimated the time occupied in the passage of light from the sun to the earth, at their mean distance apart, at 14′ 7″; Carsini, at 14′ 10″; Newton, which is very striking, much nearer to the truth, at 7′ 30″. Delambre, by taking into account, among the observations of his time, only those of the first satellite, found 8′ 13″ ·2. Encke has very justly remarked how important it would be, with the certainty of obtaining the more accordant results which the present perfection of telescopes would afford, to undertake a series of occultations of Jupiter's satellites, for the express purpose of deducing the velocity of light.

From Bradley's observations of aberration, recently discovered by Rigaud of Oxford, there follows, according to the investigation of Dr Busch of Königsberg, for the passage of light from the sun to the earth, 8′ 12″ ·14; for the velocity of the light of the stars 167,976 geographical miles in a second; and for the constant of aberration, 20″ ·2116: but, from the more recent aberration observations of Struve, made for eighteen months with the large transit instrument at Pulkowa, it appears that the first of these numbers must be considerably increased. The result of Struve's great investigation is 8′ 17″ ·78; whence with the aberration-constant, 20″ ·4451, with Encke's correction of the sun's parallax made in 1835, and with the value of the earth's semi-diameter given by him in the Jahrbuch for 1852, we have for the velocity of light 166,196 geographical miles in a second. The probable error of the velocity scarcely amounts to eight geographical miles. Struve's result for the time which light requires to reach the earth from the sun differs $\frac{1}{110}$ from that of Delambre (8′ 13″ ·2), which latter was employed by Bessel in the Tabulæ Regiomontanæ, and has been used hitherto in the Berlin Astronomical

Almanac. The discussion of this subject cannot be regarded as completely terminated; but the earlier entertained supposition, that the velocity of the light of the Pole-star was less than that of its companion in the ratio of 133 : 134, remains subject to great doubts.

A physicist distinguished for his knowledge as well as for his great delicacy in experimenting, M. Fizeau, has succeeded in executing a terrestrial measurement of the velocity of light, by means of an ingeniously devised apparatus, in which the artificial star-like light of oxygen and hydrogen is returned to the point from whence it came, by a mirror placed at a distance of 8633 metres (28,324 English feet), between Suresne and La Butte Montmartre. A disc, furnished with 720 teeth, which made 12·6 revolutions in a second, alternately stopped the ray of light, and allowed it to pass freely between the teeth of the limb. From the indications of a counter (compteur) it was inferred, that the artificial light traversed 17,266 metres (56,648 English feet), or twice the distance between the stations, in $\frac{1}{18000}$ of a second of time; whence there results a velocity of 167,528 geographical miles in a second. This result comes nearest to that of Delambre derived from Jupiter's satellites, which is 167,976 geographical miles in a second.

Direct observations, and ingenious considerations on the absence of any alteration of colour during the change of light of variable stars, (a subject to which I shall presently return), have led Arago to the conclusion that (in the language of the undulatory theory), rays of light which have different colours, and therefore very different lengths and rapidities of transverse vibration, move through space with equal velocities; but that in the interior of the different bodies through which the coloured rays pass, their rates of propagation and their refractions are different. Arago's observations have shewn, that in the prism the refraction is not altered by the relation which the velocity of light bears to that of the earth's motion. All the measurements accord in the result, that the light of the stars towards which the earth is advancing, has the same index of refraction, as the light of the stars from which the earth is receding. Speaking in the language of the emission hypothesis, the celebrated observer we have just named said, that bodies send forth rays of all velocities, but that among these different velocities there is only one which can awaken the sensation of light.

If we compare the velocities of solar, sidereal, and terrestrial light, which all comport themselves exactly in the same manner in the prism, with the velocity of the current of friction-electricity, we are inclined to assign to the latter, according to the experiments devised with admirable ingenuity by Wheatstone, a velocity superior to the former in the ratio of at least 3 to 2. According to the lowest results of Wheatstone's optical rotating apparatus, the electric current traverses 288,000 English statute miles, or 250,000 geographical miles, in a second. If, then, we reckon with Struve for sidereal light in the aberration-observations 166,196 geographical miles in a second, we get a difference of 83,804 geographical miles in a second for the greater velocity of the electric current.

This result appears to contradict the previously mentioned view of William Herschel, which regarded the light of the sun and of the fixed stars as perhaps the effect of an electro-magnetic process,—a perpetual Aurora. I say appears to contradict; for it cannot be deemed impossible that, in the different luminous bodies of space, there may be several magneto-electric processes very different in kind, and in which the light produced by the process may have a different rate of propagation. To this possible conjecture must be added the uncertainty of the numerical result obtained with Wheatstone's apparatus, which result he himself regards as "not sufficiently established and as requiring fresh confirmation," in order to be compared satisfactorily with the deductions from aberration, and satellite-observations.

Later experiments made by Walker in the United States of North America on the velocity of the propagation of electricity, on the occasion of his telegraphic determination of the longitudes of Washington, Philadelphia, New York, and Cambridge, have excited a lively interest in the minds of physical enquirers. According to Steinheil's description of these experiments, the astronomical clock of the observatory at Philadelphia was connected with Morse's writing apparatus on the line of telegraph in such a manner, that the clock's march noted itself by points on the endless strip of paper of the apparatus. The electric telegraph carries each of these points instantaneously to the other stations, and gives them the Philadelphia time by similar points on their moving strips of paper. Arbitrary signals, or the instant of the passage of a star, may be noted in the same manner by the observer, by merely touching or pressing an index with his finger. The material advantage of this American method consists, as Steinheil expresses it, "in its making the determination of time independent of the connection of the two senses, sight and hearing; as the clock's march notes itself, and the instant of the star's passage is given direct (to within a mean error of the 70th part of a second, as Walker states) by the movement of the observer's finger. A constant difference between the compared clock-marks of Philadelphia and Cambridge is produced by the time which the electric current requires to traverse twice the closed circuit between the two stations."

Measurements made with conductors 1050 English statute miles, or 968 geographical miles, in length, gave, from 18 equations of condition, the rate of propagation of the hydrogalvanic current at only 18,700 statute or 16,240 geographical miles in a second; *i. e.*, fifteen times slower than the electric current in Wheatstone's rotating disc apparatus! As in Walker's remarkable experiments two wires were not used, but half the conduction, according to the common expression, took place through the moist body of the earth, it might seem a justifiable supposition that the velocity of the propagation of electricity is dependent on the nature as well as on the dimensions of the medium. In the voltaic circuit bad conductors become more heated than good conductors, and electric discharges are very variously complicated phenomena, as appears by the latest experiments of Riess. The now prevailing views respecting what is commonly called "connection through the earth" are opposed to the view of linear molecular conduction between the two ends of the wire, and to the conjectures of impediments to conduction, and of accumulation and discharges in a current; as that which was once regarded as intermediate conduction in the earth is now supposed to belong only to an equalisation or to a restoration of electric tension.

Although, according to the present limits of exactness in this kind of observation, it is probable that the aberration constant, and therefore the velocity of light, of all the fixed stars, is the same, yet the possibility has more than once been spoken of, that there may be luminous bodies in space whose light does not reach us because, from their enormous mass gravitation constrains the luminous particles to return. The emission theory gives to such fancies a scientific form: I only allude to them here, because I shall subsequently have to notice certain peculiarities of motion ascribed to the star Procyon, which appear to point to a perturbation by dark bodies. It is the object of this part of my work to touch on matters which, during the time in which it has been in progress, have influenced the direction which science has pursued, and thus to mark the individual character of the epoch in regard to the study of nature, whether in the sidereal or the telluric sphere.—*Sabine's Edition of Humboldt's Cosmos.* Vol. iii., Part 1, p. 71.

On the Rose-Coloured Syenite of Egypt. By Professor DELESSE, Engineer of Mines. Copy communicated by the Author.

The rose-coloured syenite of Egypt is formed of *quartz*, *orthose*, *oligoclase*, *mica*, and frequently also of hornblende.*

The *quartz* is translucent and grey; it has sometimes a slight violet or smoky tint, arising, as in the quartz of protogine, from the possible presence of a small quantity of organic matter.

The *orthose* is of a pretty pure rose-colour, and reminds one of that of the orthose of the syenite of the Vosges,

* See also Lieut. Newbold on the Geology of Egypt, Quart. Journ. Geol. Soc. 1848, vol. iv., p. 340.

which, however, is of a much livelier tint; it is in crystals of some centimetres in size, which are mackled like the orthose of granite rocks; this is the most obvious, and frequently the predominating mineral of the rock, giving to it its general reddish hue. Its density is 2·568. By calcination it loses only 0·35; this loss is very slight, and such as generally takes place with orthose.

In a state of decomposition it sometimes becomes of a brownish colour, owing to the release of the small portion of oxide of manganese held in combination.

The feldspath of the sixth system has not the greasy lustre of that of the syenite of the Vosges, and it seems to me it ought to be regarded as *oligoclase;* it is most frequently white; sometimes however, it becomes yellowish, or even greenish, as is for instance observed in some specimens from Syene, and in which it is very abundant, even more abundant than the orthose.

The *mica,* rich in magnesia and iron, occurs in bright spangles, often black, but according to De Rozière, sometimes brown or green. When black, their colour resembles that of the *hornblende,* which is often associated with the mica.

There is often present *iron pyrites,* and, as in all hornblendic granites, a little oxidulated iron.

Occasionally also, but very seldom, *garnet* occurs; it is of a tarnished brown colour, and crystallized in the form of the *rhomboidal dodecahedron.*

I have determined, by the process described in the Annals of Mines (4th S., vol. xiii., p. 379), the proportions in bulk of the different minerals contained in a polished specimen of the rock, and have obtained:—Red orthose, 43—Grey quartz, 44—White oligoclase, 9—Black mica, 4.

This specimen, which was very rich in quartz, did not seem to me to contain hornblende; there was also less orthose present, and particularly less mica than might have been believed on inspection. This optical illusion is very general, and is to be ascribed to the circumstance that minerals which have lively and bright colours like orthose, and particularly mica, strike the eye much more forcibly than quartz, which has a grey and dull colour.

I have also analysed the syenite of Egypt, of which I pulverized a large piece, obtained from the Egyptian Museum in the Louvre, and placed at my disposal by M. Dubois, one of the conservators. It presented the general characters which have just been described, only that some hornblende was observed in it; I found it to contain:—Silica, 70·25—Alumina, 16·00—Oxide of iron with manganese, 2·50—Lime, 1·60—Alkalies and magnesia (diff.), 9·00—Loss by fire, 0·65 = 100·00.

When the composition of this Egyptian syenite is compared with that of the syenite of the Ballons of the Vosges, it is found to approach that of the latter.* Its proportion of silica, which is 70 per cent., is indeed the same. I have already had occasion to observe, that a syenite always containing hornblende, like that of the Ballons, may afford upwards of 30 per cent. of quartz, and that its mean richness in silica may be equal to that of many granites; this shows then, that quartz is not always, as certain geologists seem to believe, merely an accessory and unimportant element of certain syenites well-characterised, like those of the Ballons. As to the proportion of alumina in the syenite of Egypt, it is shewn by the previous experiment to be tolerably great, for it is only inferior by some hundredth parts to that of orthose; and this is accounted for by the abundance of the two feldspaths in the analysed specimen.

The proportion of iron must be particularly attributed to the mica and hornblende, both of which are rich in iron.

The proportion of lime, which is sufficiently great for a granite, is on the contrary very feeble for syenite; it is also less than that of the syenite of the Ballons, which is about 3 per cent.: this results from the presence of the oligoclase and of some hornblende.

In short, the mean chemical composition of the syenite of Egypt does not sensibly differ from that found for various granites; and indeed, as I shewed at the beginning, it generally contains much quartz. It may then be regarded as

---

* See Annals of Mines, 4th Ser., vol. xiii., pp. 688 and 693.

an hornblendic granite, or as a rock forming a passage from the family of *granite* to that of *syenite.*

From the interesting researches of Messrs Russegger* and Newbold† on the geology of Egypt, it results that the granite rocks occupy but a very small extent; they show themselves particularly at the cataract near Syene, and in the desert where they separate the Nile from the Red Sea, in the latitude of Koseir, about 26° N.

The syenite in particular is found half a league north of Syene, and according to Russegger, it extends a good deal to the south of the cataract and the island of Philæ into Nubia; and it is found at Elephantine and the intervening islands. From the collection of Lefevre it appears also to have been met with in the Djebel Gareb and Djebel Elzede (Mountain of Oil), between Koseir and Suez.

The syenite generally disappears under a brown sandstone, which according to M. Russegger is again found with the same characters in Upper Egypt, in Nubia, and in Sinai. This sandstone appears to belong to the lower part of the chalk formation, or the Quader-sandstein: near Fatireh it is covered with a white earthy chalk, having a somewhat conchoidal fracture, that reposes on it in conformable and horizontal strata.

The quarries in which the ancients worked the syenite have been observed by all travellers who have visited Egypt; they are principally south of Syene, and between Syene and the island of Philæ. The detached blocks of the syenite, near the cataract, are sometimes of a spheroidal form, and are separable into concentric layers; according to Newbold, however, the dry and hot climate of Egypt preserves the granite rocks much more from decomposition than the climate of India.

M. Russegger remarks, that near the cataract, the blocks found in the river, or at a small distance from it, are covered with a very thin and brilliant substance resembling blackish

---

* Russegger, Reisen in Europa, Asien, und Afrika, U. S. W. Stuttgart, 8vo.

† *Loc. cit.*

brown pitch. This coating, so strongly united with the rock as to be quite inseparable, is considered by M. Russegger to be oxidulated iron.*

Messrs Russegger and Lefevre were struck by the fact, to which they frequently afterwards alluded, that the syenite of Egypt is traversed by a multitude of large veins of diorite, which is particularly the case along the cataract, near Philæ, in the neighbourhood of Syene, &c.; these diorite veins are however well known, for they also were worked by the Egyptians.†

This association of syenite and diorite is not accidental; and I have made similar observations on all the syenites that I have studied *in situ*. Indeed I could almost always find that they were associated with diorites. Thus, at the Vosges especially, the syenite of the Ballons is accompanied by diorites, which are sometimes at the bottom, sometimes on the sides of the Ballons of Alsace and Conté, forming either veins, fairly separated from the syenite and enclosed by it, or dykes uniting with and insensibly passing into the syenite.

It would hence appear, that the development of hornblende in the syenite is in intimate relation with the contents of the veins of diorite enclosed therein, and subsequently metamorphosed into hornblende.

It is however, necessary to add, that if the syenite be generally associated with diorite, the contrary is not always the case; also, if a diorite form a vein in a granite, it must not thence be concluded that crystals of hornblende had been therein developed by that circumstance alone, and that such granite had been metamorphosed into syenite; at the Vosges, for instance, the granite is sometimes traversed by veins of diorite, and yet is not hornblendic.

The Egyptians made great use of syenite; subsequently it was worked by the Greeks, and after that by the Romans. The syenite of Egypt is still sometimes employed instead of marble, and its price may be approximately estimated at 200 francs per square metre polished; it is brought as ballast by

---

* Russegger, vol. ii., p. 321.

† Ibid., vol. ii., pp. 320, 322, 326, &c.

vessels trading with Alexandria, and is designated in commerce by the name of the *Eastern Red Granite.**

Great quantities of fragments of syenite are found in the ruins of all the ancient towns of Egypt; and the imagination is really tasked when thinking of the difficulties presented in the cutting, polishing, and transporting of so many gigantic monuments. The most celebrated of these are, according to De Rozière, those of the isles of Philæ and Elephantine, those of Thebes, Luxor, Heliopolis, and especially of Alexandria; and although the syenite was extracted in the country surrounding Syene, yet the fragments are more and more abundant the further we descend the Nile towards the north; which is to be ascribed, as M. De Rozière has shown, to the fact that the seats of government necessarily approached the Mediterranean, and that the requisite material for the numerous sacred and palatial structures was wanting in that northern region of Egypt which is essentially calcareous and gravelly.

The syenite was of all rocks the one preferred by the Egyptians, and they employed it for the construction of their most remarkable monuments: of these monuments there might be cited the obelisks, the sphinxes, the sarcophagi found in all parts of Egypt, Pompey's Pillar, and Cleopatra's Needle, at Alexandria, both the inside and outside of the great pyramid of Cheops, and particularly the monolith sanctuary of Sais. At Paris may be seen one of the Luxor obelisks, and in the Egyptian Museum at the Louvre, the feet and head of a colossal statue of Amenophis III., as well as a great number of sculptures, which under the ever pure sky of Egypt, for the greater part have not suffered any alteration, even perfectly preserving their polish, for nearly 4000 years.†

* Brard, Mineralogie appliquée aux Arts, t. ii., p. 241.

† The Monumental Egyptian syenites of the British Museum are well known.

On the Analogy between the mode of Reproduction in Plants and the "Alternation of Generations" observed in some Radiata.

The very remarkable fact that a Polyp and a Medusa may be in some instances different states of one and the same species, has been well established of late by the researches of Sars, Dalyell, Steenstrup, and others; and recent important observations have been made on the subject by Professor Agassiz. The alternations are as follows:—

1. The Medusa produces eggs:—

2. The eggs, after passing through an infusorial state, fix themselves and become polyps, like Corynæ, Tubulariæ, or Campanulariæ:—

3. The polyps produce a kind of bud that finally drops off and becomes a Medusa.

Thus the egg of a Medusa, in such cases, does not produce a Medusa, except after going through the intermediate state of a polyp.

Or if we commence with the polyp, the series is thus:—

1. The polyp produces bulbs that become Medusæ.

2. The Medusæ produce eggs.

3. The eggs produce polyps.

This is what is called by Steenstrup "Alternation of Generations;" and he considers the earlier generation as preparing the way for the latter. It certainly seems to be a most mysterious process:—a parent producing eggs which afford a progeny of wholly different form (even so different, that naturalists have arranged the progeny in another grand division of the Radiata); and this progeny, afterwards, by a species of budding or germination repeating the form of the original parent.

Yet although seemingly so mysterious, is not this mode of development common in the vegetable kingdom? Is it not the prevalent process in the plants of our gardens, and fields, with which we are all familiar?

It is well known to us, that in most plants, our trees and shrubs for example, growth from the seed brings out a bud of leaves; from this bud after elongation, other leaf-buds are often developed, each consisting like the first of a number of leaves. It is an admitted fact (as may be found in Treatises on Vegetable Physiology), that each of these buds is a proper plant-individual, and that those constituting a tree are as distinct and independent as the several polyps of a compound zoophyte; and that the tree therefore is as much a compound group of individuals, as the zoophyte. In some cases the plant forms but a single leaf-bud; in others, where there is successive germination for a period, the number is gradually multiplied, and more or less according to the habit of the species. So among polyps, there are the simple and compound Tubularia, Campanularia, and the like.

After the plant has sufficiently matured by the production and growth of its number of leaf-buds, there is a new development—a flower-bud—consisting of the same elements as the leaf-bud, but wholly unlike it in general appearance—as much so, as the Medusa is unlike the polyp. The flower individual starts as a bulb from the leaf individual, or the group of leaf individuals, and is analogous in every respect to the bulbs from the Campanulariæ and allied species; and when it has fully matured, it produces, like the Medusa, ovules, or seed—these seed to begin the round again of successive or alternating developments.

Thus among plants the seed produce leaf individuals; and these produce seeds; precisely, as the egg produces polyps, the polyps bulbs, that develop into Medusæ, and the Medusæ eggs.

When we follow out this subject minutely, we find the analogy completely sustained even in minor points of structure and growth. The leaf-bud consists of leaves developed in a spiral order; and in the polyp, as some species show beyond doubt, the tentacles and corresponding parts are spiral in development. The same spiral character is found in the flower, but the volutions are so close as not to be distinguished readily from circles. In the Medusæ referred to, the regularly circular form is far more neatly and perfectly developed than among the polyps—as is clearly seen in a comparison of the polyp Coryna, with the elegant Sarsia, a species of which is described and beautifully delineated in Professor Agassiz's recent memoir, published by the American Academy of Arts and Sciences at Boston. The relations in structure between plants and polyps might be further dwelt upon; but for other observations the writer would refer to his volume on Zoophytes.

The only point in which the analogy seems to fail, is that the Medusa bud falls off before its full development, while this is not so with the plants. But it is obvious that this is unimportant in its bearing on this subject. It is a consequence of the grand difference in the mode of nutrition in the two kingdoms of nature; for the plant-bud on separation loses its only means of nutriment.

The law of alternating generations is therefore no limited principle, strange and anomalous, applying only to a few Radiata. It embraces under its scope, the vegetable kingdom, and it is but another instance of identity in the laws of growth in the two great departments of life.—*Dr W. B. Carpenter and James D. Dana.*

Professor OWEN *on Metamorphosis and Metagenesis.* Being abstract of a Lecture delivered by him at the Royal Institute of Great Britain in February 1851.*

The Lecturer commenced by passing under review the Linnæan characters of Minerals, Vegetables, and Animals, and the subsequent distinctions which had been proposed for the discrimination of the two latter kingdoms of nature. After discussing those founded on motion, the stomach, the respiratory products, the composition of the tissues, and the sources of nourishment, it was shewn that none of these singly, define absolutely the boundaries between plants and animals; it requires that a certain proportion of the supposed characteristics should be combined for that purpose.

The individuals in which such characters are combined are specially defined members of one great family of organised

---

* The above important abstract was communicated to us by the author at our request.—EDIT.

beings, and the supposed peculiarly animal and vegetable characters taken singly, interdigitate, as it were, and cross that debatable ground and low department of the common organic world from which the specialised plants and animals rise; and there are numerous living beings with the common organic characters that have not the distinctive combined superadditions of either group.

Between the organic and inorganic worlds the line of demarcation may be more definitely drawn. The term "growth" cannot be used in the same sense to signify the increase of a mineral and of an organism. The mode of increase is different: there is a definite limit to it in the organic kingdom, and something more than mere growth takes place in the progress of an organism from its commencement to maturity. This was exemplified by reference to the human subject, to the lion which acquires its mane, to the stag which gets its horns, and to the change of plumage in birds during the course of growth. The changes of form and character are still more remarkable in the kangaroo; and in the frog they are such as to have received the name of "*metamorphosis.*"

The development of the frog was traced to its exclusion from the egg in the form of a fish, with external gills, a long caudal fin, and without legs.

The internal skeleton, like the external shape, is adapted for aquatic life.

Only those parts are ossified which are to be retained in the mature state. The vertebræ are at first biconcave, as in fishes, with intervening spherical elastic balls filled with fluid: they are converted into ball and socket joints by the ossification of the sphere, and its anchylosis to the back of the vertebræ. The pelvis and hind legs are progressively developed; and, whilst this change is proceeding, the tail is undergoing proportional absorption. The chief change in the skull of the larva is operated in the lower or hæmal arches and their appendages. The maxillary arch is widened and provided with teeth, and the horny mandibles are shed. The mandibular arch retrogrades as well as expands. The hyoidean undergoes a remarkable change of size and shape, and the branchial arches are absorbed, excepting a small portion which is converted into the hinder " horns" of the hyoid for supporting the larynx.

The scapular arch, which at first was connected with the occiput, whilst supporting the branchial heart—its primary function, begins as soon as the fore-legs bud out, to retrograde, and the sternum is developed to complete the *point d'appui* for the fore-limbs.

The food of the larva is chiefly the soft decaying parts of aquatic plants ; it has a horny beak, a long alimentary canal disposed in a series of double spiral coils : but, as its frame undergoes the changes adapting it for life on land, and a purely animal diet, the mandibles are converted into jaws and teeth, and the long spiral intestine into a short and slightly convoluted one.

Soon after the external gills have reached their full development they begin to shrink and finally disappear ; but the branchial circulation is maintained some time longer upon internal gills : by anastomoses between the principal branchial vessels these are converted into the aortic arches, carotids and subclavians ; the internal gills with the cartilaginous hoops supporting them are absorbed, and lungs and glottis for breathing the air directly are developed.

Thus an animal formed for moving in water is changed into one adapted for moving and leaping on land ; a water-breather is converted into an air-breather ; a vegetable feeder into a carniverous animal : yet the series of transmutations are limited to the nature of the species and produce no other. The frogs that croak in our marshes are as strictly batrachian as those that leapt in Pharaoh's chamber ; their metamorphoses have led to nothing higher than their original condition, as far as history gives us any knowledge of it. With each successive generation the series of changes recommences from the old point, and ends in a condition of the animal adapted to set the same series again on foot.

Having traced the principal stages in the metamorphosis of an animal from a swimmer to a leaper ; the Lecturer next took an instance where one that begins life as a burrower or a crawler is converted into an animal of rapid and powerful flight.

Most insects quit the egg in the form of a worm, which masking, as it were, a different and higher form, is called the "larva;" it is active and voracious,—but usually falls into a kind of torpor during which the changes take place which issue in the flying insect; during the passive stage of metamorphosis it is called a "pupa;" the last volant stage is the "imago."

The chief steps in the metamorphosis were traced as they affect the outward form, the digestive organs, the circulatory, and respiratory, and nervous systems.

The main differences in the metamorphoses of insects relate to the place where, and the time during which they are undergone. The young cockroach and the little aphis, which were first acephalous and apodal, and had then thirteen equal segments, with soft unjointed legs, proceed to acquire a distinct head with antennæ, a thorax with three pairs of long jointed legs, and an abdomen, before they quit the egg; they thus enter upon active life under the guise of a crab, instead of a worm. With regard to the *Aphis*, that insect, instead of proceeding to perfect its individual development, may at once begin the great business of its existence by parthenogenetic procreation. Bonnet's experiments, which first brought to light this marvellous fact, have received uniform confirmation from all subsequent inquirers, and no natural phenomenon is now better determined.

From seven to eleven successive generations have been traced before the individual has finally metamorphosed itself into the winged male or winged oviparous female.

In autumn, when the nights grow chilly and long, the oviparous imago completes her duty by depositing the eggs in the axils of the leaves of the plant, where they are protected from the winter frost, and ready to be hatched at the return of spring. Then recommences the cycle of change, which being carried through a succession of individuals and not completed in a single life-time, is a "metagenesis" rather than a "metamorphosis."

This phenomenon which, until very recently, was deemed an exception, and a most marvellous one, in Nature, now proves to be an example of a condition of procreation to which the greater part of organised Nature is subject.

The Lecturer was inevitably limited in his choice of illustrations; and proceeded to an instance of metagenesis from the radiated sub-kingdom of animals.

The stages of this metagenesis have been best and most completely traced in the *Medusa aurita*, by Siebold, Dalyell, Sars, and others.

The first step was made by Siebold, who, in 1839, traced the development of the *Medusa aurita* from the egg to a stage resembling a ciliated monad, then to a lobed rotifer, and next to a long-armed polype.

This polype stage of the *Medusa* had been previously recognised in 1788, but without a suspicion of its true nature, by O. F. Müller, who called it *Hydra gelatinosa*.

It was next observed, and its habits more fully described, by Sir John Dalyell, in 1834, as *Hydra tuba:* and in 1836 he made known the singular metamorphoses into forms which Sars had previously described as *Scyphistoma* and *Strobila;* and Dalyell saw the spontaneous division of the latter into a pile or series of small Medusæ. All the stages of the metagenesis were independently noted by Sars who described them in 1841.

The difficulty of accounting for the presence of Entozoa in the interior parts of animal bodies is rapidly disappearing as the knowledge of their course of development advances.

The principal stages of this development were described in a small worm (*Monostoma mutabile*), parasitic in the air-cells, intestines and peritoneal cavity of many water-fowl.

The ovum is converted into a ciliated monadiform embryo; which escapes from the bird, and swims about freely in the water. A clear mass may be discerned in the interior which exhibits independent movements. This body is liberated, grows rapidly, and generates in its interior a number of independent organisms provided with a cephalic spiculm and a caudal appendage, referable by their form to the genus *Cercaria*. They are very active and insinuating, could even bore through the skin by the sharp needle-like armature of the head, and somehow or other do, under the guise of the *Cercaria,* again get access to the interior of the water-fowl: fall into a state of torpor; become circular flattened pupæ; and are finally metamorphosed into monostomes—a sluggish pendant parasite utterly deprived of the power of existing in water, or of gaining access, as a monostome, to the interior of any animal.

Steenstrup, who has the merit of having first grouped together and pointed out the analogies of the different stages in the animals that undergo these successive changes, generalizes the facts under the phrase of 'Alternate Generation,' and he calls the procreant larvæ 'Amme,' or Nurses, and 'Gross-amme,' or Grand-nurses. There is no particular objection to these names; but we naturally desire to know on what power the metageneses depend.

Professor Owen thought the key to the power was afforded by the process which the germinal part of every egg undergoes before the embryo begins to be formed.

A principle, answering to that of the pollen, which fertilizes the seed of plants, is the efficient cause of these changes; its mode of operating is best seen in the transparent eggs of some minute worms; the principle manifests itself as a transparent, highly refractive globule in the centre of the egg: it then divides; and each division, attracting the vitelline matter of the egg about it, divides that matter into two parts. This division is repeated with the same result, until the principle has diffused itself by indefinite multiplication through the whole yelk which then constitutes the 'germ-mass.'

The next stage is the formation of the embryo: certain of the minute subdivisions, called 'nuclei' or nucleated cells, combine and coalesce to constitute the tissues of the embryos: they are afterwards incapable of generating. If all be so metamorphosed the organism cannot procreate of itself; but if a part only of the germ-mass be metamorphosed into tissues the unchanged remnant may, if nutrition, heat and other stimuli are present, repeat the same actions as those that formed the first germ-mass, and lay the foundation of future embryos.

In proportion to the amount of the substance of an organism which retains the primitive condition of cells, is the power of producing new individuals without receiving a fresh supply of the pollen-principle.

Thus in a plant, when the seed has received the matter of the pollen-filament, analogous changes take place to those that have been described in the animal egg, and the embryo plant appears in the form of the cotyledonal leaf with its radicle or rootlet. From this shoots forth another leaf with its stem: and the cellular substance of the pith with its share of the pollen-principle goes on developing fresh leaves and leaf-stalks; until a provision for developing fresh pollen is made by transforming certain individual leaves into a higher form of the 'phyton' or elemental plant. Thus a generation or 'whorl' of leaves assumes the character of sepals, another that of petals, a third that of stamens, a fourth that of pistils: and in the two latter forms we recognise the analogues of the perfect male and female of the animal.

The development of the compound polype follows very closely the stages of the compound plant, which we call shrub or tree: the ovum, like the seed, having received the pollen-principle, is converted into countless cells and nuclei of cells by the process for diffusing that principle through, or of assimilating it with, the matter of the egg. Then certain germ-cells are metamorphosed into a ciliated integument, and the larva starts forth, in a state answering to the cotyledonal leaf of the plant; the ciliated larva settles, subsides, and shoots up a stem from which a digestive polype is developed, answering to the leaf: but the pollen-force not being exhausted, a second branch and polype are developed, and so on until a preparation is made for a fresh supply of pollen-force, by metamorphosing the polype into a higher form of individual; and this, in many compound polypes, is set free in the shape of a minute medusa.

The true nature and relation of the individual polype to the compound whole is well illustrated by the propagations of the Aphides.

By comparing with the diagrams of the metagenesis of the plant and polype, that of the Aphis, in which was represented the corresponding stages intervening between the ovum and the perfect male and female individuals of the *Aphis*, the analogy between these stages in the plant, the polype and the insect, was shewn to be both true and close. The microscopic seminal filament of the male *Aphis* answers to the microscopic pollen filament of the male leaf or "stamen." The ovum of the female *Aphis* to the ovule of the female leaf or pistil: by their combination the fertile ovum results. The same process of cell-formation ensues, and the embryo *Aphis* is formed by the combination and metamorphoses of certain of these secondary germ-cells; but it retains the rest unchanged in its interior, which may be compared with the cells of the pith of the plant, and with the cells in the corresponding more fluid part of the pith of the polype. Under favourable circumstances of nutriment and warmth, certain of these cells repeat the process of embryonic formation, and a larval individual like that from the ovum is thus reproduced; which is only not retained in connection with its parent, because the integument is not co-extended with it.

The generation of a larval Aphis may be repeated from seven to eleven times without any more accession to the primary pollen-force of the retained cells than in the case of the zoophyte or plant; one might call the generation, one by "internal gemmation," but this phrase would not explain the conditions essential to the process, unless we previously knew those conditions in regard to ordinary or external gemmation.

At length, however, the last apterous or larval Aphis, so developed, proceeds to be "metamorphosed" into a winged individual, in which either only the fertilizing filaments are formed, as in the case of the stamens of the plant, or only the ovules, as in the case of the pistil. We have, in fact, at length "male and female individuals," preceded by procreative individuals of a lower or arrested grade of organization, analogues to the gemmiparous polypes of the zoophyte and to the leaves of the plant.

The process was described for its better intelligibility in the Aphides as one of a simple succession of single individuals, but it is much more marvellous in nature. The first-formed larva of early spring procreates not one but eight larvæ like itself in successive broods, and each of these larvæ repeats the process; and it may be again repeated in the same geometrical ratio until a number which figures only can indicate and language almost fails to express, is the result. The Aphides produced by this internal gemmation, are as countless as the leaves of a tree, to which they are so closely analogous.

It generally happens that the metamorphosis which has been described as occurring after the seventh or eleventh generation takes place much earlier in the case of some of the thousands of individuals so propagated; just as a leaf-bud near the root may develope a leaf-stem and a flower with much fewer antecedent generations of leaves from buds than have preceded the formation of the flower at the summit of the plant; or just as one of the lower and earlier formed digestive polypes may push out a bud to be transformed into a procreative and locomotive polype. The same analogy is closely maintained throughout.

The wingless larval Aphides are not very locomotive; they might have been attached to one another by continuity of integument, and each have been fixed to suck the juices from the part of the plant where it was brought forth. The stem of the rose might have been incrusted with a chain of such connected larvæ as we see the stem of a fucus incrusted with a chain of connected polypes, and only the last developed winged males and oviparous females might have been set free. The connecting medium might even have been permitted a common current of nutriment contributed to by each individual to circulate through the whole compound body. But how little of anything essential to the animal would be affected by cutting through this hypothetical connecting and vascular integument and setting each individual free! If we perform this operation on the compound zoophyte, the detached polype may live and continue its gemmiparous reproduction. This is more certainly and constantly the result in detaching one of the monadiform individuals which assists in composing the seeming individual whole called "Volvox globator;" and so likewise with the leaf-bud. And this liberation Nature has actually performed for us in the case of the Aphis, and she thereby plainly teaches us the true value or signification in morphology of the connecting links that remain to attach together the different gemmiparous individuals of the volvox, the zoophyte, and the plant.

The analogy between the procreating larvæ of the *Aphis*, the *Medusa*, and the *Coralline* is so true and so close, that if the larval Aphis be a distinct individual and not a part, so must be the strobila, the planula, and the gemmiparous leaf: if the succession of larval Aphides be truly described, as a succession of generations, so must that succession of planula, polype and strobila which leads to the oviparous Medusa: and that succession of planulæ and nutritive polypes which precede the detachment of the free procreative medusoid polypes in the Coryne; and the like with the plant generations preceding the flower.

It would have been easy, if time permitted, to multiply the illustrations of the essential condition of these phenomena. That condition is, the retention of certain of the progeny of the primary fertilised germ-cell, or in other words, of the germ-mass, unchanged in the body of the first individual developed from that germ-mass, with so much of the pollen force inherited by the retained germ-cells from the parent-cell or germ-vesicle as suffices to set on foot and maintain the same series of formative actions as those which constituted the individual containing them.

How the retained pollen-force or seminal-force operates in the formation of a new germ-mass from a secondary, tertiary, or quaternary derivative germ-cell, the Lecturer did not profess to explain; neither was it known how it operates in developing the primary germ-mass.

The botanist and physiologist congratulates himself with justice when he has been able to pass from cause to cause, until he arrives at the union of the pollen-filament with the ovule as the essential condition of development—a cause ready to operate when necessary circumstances concur, and without which those circumstances would have no effect.

The chief aim of the present discourse was to point out the circumstances which bring about the presence of the same essential cause in the cases of the development of the successive generations completing the metagenetic cycle of the Aphis, the Medusa, the Polype, and the Entozoon. The cause is the same in kind though not in degree, and every successive generation, or series of spontaneous fissions, of the primary germ-cell must weaken the pollen-force transmitted to such successive generations of cells.

The force is exhausted in proportion to the complexity and living powers of the organism developed from the primary germ-cell and germ-mass. It is consequently longest retained and furthest transmitted in the vegetable kingdom; the zoophytes manifest it in the next degree of force; and the power of retained germ-cells to develope a germ-mass and embryo by the remnant of the pollen-force which they inherited, is finally lost, according to present knowledge in the class of Insecta and in the lower Mollusca.

Chemical Examination of Specimens of Purple Copper Ore and Copper Pyrites, with Remarks on their Constitution. By Mr DAVID FORBES, of the Espedal Copper Works, Norway. Communicated by the Author.

Although the two ores of copper which form the subject of the present notice have frequently been analyzed, still the following details may be considered not entirely devoid of interest, both as confirming the results of previous chemists and as probably also assisting to determine their chemical constitution, which has often been a matter of discussion.

The ores in question were analyzed at the request of his Royal Highness the Crown Prince of Sweden and Norway, and obtained from the Gustav and Carlstad copper mines, situated in the county of Jemtland in Sweden.

I. *Octohedral or Purple Copper Ore.*—Free from gangue, and apparently in a quartzy matrix. Metallic lustre; bronze colour when recently fractured, but soon tarnished to a fine purple or purple-red tint. Streak greyish black. Fracture conchoidal, brittle, and when pulverized gave a bronze-brown powder. Hardness about that of fluorspar, or 4° of Mohs' scale. The specific gravity taken at 60° F. was 4·432, water being 1. The specific gravity generally assigned is

5·03, and hardness as 3°, but the difference may in some degree be influenced by the greater per-centage of silica contained in the specimen. The method of analysis employed was as follows:—20·58 grains weighed into a small glass tube were placed in a flask with 1½ oz. nitroso-nitric acid, closed by a caoutchouc cork, and allowed to stand twelve hours. It was then heated for some time until all sulphur was dissolved, with the exception of a small round globule, which was of a fine yellow colour, and was removed, washed, dried, and weighed. As after ignition it left no residue, it was regarded as pure sulphur. A quantity of white silicious matter, apparently quartz, also remained, and was collected on a filter, washed, dried, incinerated, and weighed.

The solution was now considerably diluted, and precipitated, whilst boiling by chloride of barium, and the resulting sulphate of barytes filtered off, washed, dried, and incinerated.

The excess of barytes in the solution was now removed by sulphuric acid, and removed by filtration.

The copper was now precipitated by a stream of sulphuretted hydrogen, the precipitate being washed with water previously boiled and saturated with that gas. The sulphuret of copper was now dried and ignited for a very long time with excess of air, so as to remove all possible sulphur, and then in the same crucible treated with nitric acid, evaporated to dryness, and strongly ignited, so as to decompose any sulphate of copper which perchance had been formed, then cooled over sulphuric acid, and weighed. The filtrate from which the copper had been separated was now boiled to expel all sulphuretted hydrogen, and oxidized by addition of chlorate of potash; then precipitated by caustic potash, and the precipitate dissolved off the filter by hydrochloric acid, neutralized by ammonia, and then precipitated by succinate of ammonia, observing the usual precautions, in order to separate any manganese present. The succinate of iron, after being washed on a filter with warm ammonia water to remove the succinic acid, was re-dissolved in hydrochloric acid, precipitated by ammonia, and determined as oxide, using the precaution to moisten the oxide after ignition, with nitric acid, and again to apply heat, so as to ensure that no de-oxidation could take place.

The solution from which the iron had been separated was now precipitated by carbonate of potash, but the resulting oxide of manganese precipitated was so small as hardly possible to determine it with exactness, and was therefore not collected. It was, however, sufficient to prove the existence of a trace of manganese in the ore, which possibly would amount to about ·1 per cent.

The quantities found were as follows :—

	Mineral employed,	.	20·58	grains.
Obtained	Sulphate of barytes,	.	26·77	„
„	Free sulphur,	.	1·36	„
„	Sesquioxide of iron,	.	3·28	„
„	Silicious residue,	.	·79	„
„	Oxide of copper,	.	15·39	„

And the per-centage composition calculated from these results is as follows :—

Sulphur,	5·04	and per cent.	24·49
Copper,	12·28	„	59·71
Iron,	2·29	„	11·12
Manganese,	trace	„	about ·1
Silica,	·79	„	3·83
Loss,	·18	„	·85
	100·00		100·00

If we now deduct the silicious matter and loss in analysis from the amount employed, and allow the deficiency to be equally distributed over the other constituents, we have as the composition of the pure sulphuret—

Sulphur,	.	25·69
Copper,	.	62·64
Iron,	.	11·67
		100·00

For the sake of comparison I subjoin the following analyses, which give almost the same results :—

	Hisinger.	Plattner.	Bodemann.
Sulphur, .	24·696	25·058	25·70
Copper, .	63·334	63·029	62·75
Iron, .	11·804	11·565	11·64
Silica, .	0·166	0·000	0·04
	100·000	99·652	100·13

The mineral analyzed by Hisinger is of unknown locality, that of Plattner from the White Sea, and that of Bodemann from Bristol in the United States. All these are evidently of the same chemical constitution as the specimen above analyzed, and may be represented by the formulæ 7 Cu_2 S + Fe_3 S_4, or by Cu_2 S + Fe_2 S_3. Their formulæ give respectively the following per-centage composition :—

	7 Cu_2 S + Fe_3 S_4.	Cu_2 S + Fe_2 S_3.
Sulphur, .	25·23	25·77
Copper, .	63·17	63·37
Iron, .	11·60	10·86
	100·00	100·00

The last of these formulæ is that assigned by Bodemann to the mineral analyzed by him, but the first approximates more nearly to the per-centage composition actually found.

Although it is evident that one or other of these two formulæ will represent the mineral here analyzed, as well as the three analyses above quoted, still it is impossible to reconcile these results with many other analyses of this mineral given by other chemists, as, for example, the following :—

	I.	II.
Sulphur, .	28·23	23·75
Copper, .	56·76	61·07
Iron, .	14·84	14·00
Silica, .	0·00	0·50
	99·33	99·32

The first of these analyses being by Plattner refers to a specimen from Cornwall in England, which was crystallized, and could be represented by the formula 3 Cu_2 S + Fe_2 S_3, as given by him. The latter analysis by Philipps, from Killarney in Ireland, will be found to be nearly the formula 2 Cu_2 S + Fe S. These two formulæ give the following per-centage results :—

	$3 Cu_2 S + Fe_2 S_3$.	$2 Cu_2 S + Fe S$.
Sulphur, .	28·33	23·89
Copper, .	55·74	62·68
Iron, .	15·93	13·43
	100·00	100·00

Klaproth has likewise analyzed two specimens of this mineral, but gives also oxygen as a constituent. However, from the less perfect means of analysis employed by him, and the circumstance that the results on adding make up exactly the 100, it seems very doubtful as to there having been any oxygen whatever in these specimens, and most probable that the loss which occurred in analyzing them had by him been regarded as oxygen, which has not been found by any succeeding chemists.

Whilst the results of Klaproth's analyses are therefore introduced, I have likewise placed by their side the quantities which would result after calculation on the supposition that no oxygen was present, and that the deficiency thus produced was distributed equally over all the three constituents.

	From Norway.	Do. Corrected.	From Silesia.	Do. Corrected.
Sulphur, .	19·0	20·00	19·0	19·79
Copper, .	58·0	61·06	69·5	72·39
Iron, .	18·0	18·95	7·5	7·82
Oxygen, .	5·0	?	4·0	?
	100·0	100·00	100·0	100·00

From the analyses of this mineral which we have here given, it will at once be seen that the results are so completely at variance with one another, that it is impossible to bring them under any distinct formula. There are also other analyses equally different, which, from fear of occupying too much space, I have been obliged to leave out; and although Plattner has considered it probable that there are three different species included under this mineral, still it would require much more than double this number to reconcile all the analyses which differ so essentially from one another.

It must likewise be remembered, that if we adopt Plattner's supposition, and permit all these to be regarded as distinct species, then, as their physical and crystallographic characters are apparently perfectly identical, we should have a number of mineral species which it would be impossible to distinguish from one another, otherwise than by a complete chemical analysis, which the majority of mineralogists are either incapable, or do not possess the opportunity of doing.

It has therefore been my opinion, on consideration of these circumstances, that the mineral in reality cannot be regarded as a combination of various sulphurets of copper with sulphurets of iron, but that the iron replaces the copper; for although we have not as yet had these two metals proved isomorphous when in combination with sulphur, still we have many reasons for believing this to be the case; and we know that in their free state, as well as in combination with oxygen, iron and copper are isomorphous,* and we therefore can, without doubt, regard those minerals as combinations of copper with sulphur, the iron playing a secondary and non-essential part.

As, however, the per-centage of sulphur is not constant, it would appear that two sulphurets of copper had come into play, and that the purple copper ore could be considered to consist of a compound of the disulphuret of copper (in which the iron replaces more or less of the copper) with protosulphuret of copper,—the proportion of the latter sulphuret in the compound varying without essentially altering the physical properties of the mineral.

On taking this view of its constitution, it will likewise follow, that iron replaces the copper in the native disulphuret of copper, and likewise in the mineral Digenite; and as these come in contact with the proposed classification, it will be necessary to pay attention to them at the same time.

---

* Frankenheim has likewise considered this to be the case in the following combinations, as in the grey copper ore and in Tennantite, making its formula $(Cu_2\,S, Fe\,S,)^4\,As_2\,S_3$, instead of, as usually given $(Fe\,S, Cu_2\,S)^4\,As_3\,S_3 + 2\,Cu_2\,S^4\,As_2\,S_3$; also in the case of silver Fahlerz making $(Cu_2\,S, Ag\,S, Fe\,S, Zn\,S,)^4\,(Sb_2\,S_3, As_2\,S_3)$, instead of $(Fe\,S, Zn\,S)^4\,Sb_2\,S_3 + 2\,(Cu_2\,S\,Ag\,S)^4\,Sb_2\,S_3$, assigned to it by Rose.

According to this classification, we should have only two distinct minerals, namely,—

1. The disulphuret of copper (vitreous copper ore, or glance copper), under which we at the same time include the two minerals analyzed by Klaproth, and apparently regarded by him, though erroneously, as purple copper ore. For the sake of comparison, we not only repeat these two analyses, but also add two analyses of the ordinary vitreous copper ore, in one of which it will be seen that iron is present in greater quantity than usually ascribed to accidental impurity, and apparently replaces some part of the copper.

	Klaproth's Analyses corrected.		Vitreous Copper Ore.	
	I.	II.	Klaproth.	Thomson.
Sulphur, .	20·00	19·79	18·50	20·62
Copper, . .	61·06	72·39	78·50	77·16
Iron, . . .	18·95	7·82	2·25	1·45
Silica, . .			·75	
	100·00	100·00	99·00	99·23

The formula Cu_2 S will give the pure disulphuret to be composed of—

Sulphur, ...	20·27
Copper, ...	79·73
	100·00

which closely agrees with the above results.

2. The purple copper ore, considered as a compound of the disulphuret with the protosulphuret of copper in varying proportions, and with iron replacing more or less of the copper in the disulphuret. The analyses previously given can be represented by the following subordinate formulæ:—

(*a*) $2\,Cu_2\,S + Cu\,S$, as exemplified in the mineral analyzed by Philipps from Killarney in Ireland. This formula gives sulphur 23·38, copper (with iron) 76·62 = 100.

(*b*) Formula $Cu_2\,S + Cu\,S$, as in the mineral here noticed, as well as those analyzed by Bodemann, Hisinger, and Plattner, giving sulphur 25·31, copper (with iron) 74·69 = 100.

(*c*) Formula $Cu_2S + 2 CuS$, represented by the crystalline variety analyzed by Plattner. Likewise I must regard the mineral called Digenite, discovered by Breithaupt, and analyzed by Plattner, as belonging to this class, being the pure sulphuret, without replacement of iron.

The per-centage results are as follow :—

	Formula $Cu_2S+2CuS$.	Digenite.*
Sulphur, .	27·60	29·0
Copper, .	72·40	71·0
	100·00	100·0

We have thus included the whole of those minerals under a general formula, which is apparently simpler than that we should give each one of them, so similar as they are in physical properties, a distinct and varying formula, differing so much from each other, that we could never admit of their being associated together in any general chemical series; whereas by adopting the above view, we have at once both a systematic classification, and some reasons for the different results obtained by the several analysts. It must, however, be left to other chemists and mineralogists to give a more decided opinion on, and to examine more carefully into, the correctness of this hypothesis.

II. *Yellow Copper Pyrites.*—The specimen massive and apparently free from all admixture of earthy matter. Metallic lustre, fine yellow colour, not easily tarnished. Streak greenish-grey, and when powdered gave a dark green powder. Cleavage indistinct. Hardness about 3·5; and specific gravity, taken at 60° Fahrenheit, was 4·185.

The method of analysis employed was nearly the same as described under the preceding mineral, with the exception that the solution was effected by heating it with nitric acid and chlorate of potash, until the sulphur separated of a clear yellow colour. A fresh portion of the mineral was employed to determine the bases. The quantities employed and found were as follow :—

* The analysis of Digenite here given is taken from "Naumann's Elemente der Mineralogie," p. 402, as I have not seen the original results by Plattner.

Mineral used for sulphur determination, .	12·39	grains.
Free sulphur obtained, ...	1·11	,,
Sulphate of barytes do. . . .	22·43	,,
Silica, ...	·04	,,
Mineral employed for determining the bases,	10·85	,,
Protoxide of copper obtained, . .	4·45	,,
Sesquioxide of iron do., . .	5·08	,,

which results give the following per-centage composition—

Sulphur, ...	33·88
Copper, ...	32·65
Iron, ...	32·77
Manganese, . . .	trace, probably = ·02
Silica, ...	·32
Loss, ...	·38
	100·00

and after deducting the silica and loss, will leave a composition of the pure sulphuret,

Sulphur, ...	34·11
Copper, ...	32·88
Iron, ...	33·11
	100·00

The formula generally assigned is, $Cu\,S + Fe\,S$, or, according to Rose, $Cu_2\,S + Fe_2\,S_3$, both of which will give the same composition as under.

Sulphur, ...	35·38
Copper, ...	34·71
Iron, ...	29·83
	100·00

Most analyses of pure varieties do not differ much from this, yielding from 34 to 37 per cent. sulphur, 30 to 34 per cent. copper, and 30 to 33 per cent. iron, and they can without difficulty be all reconciled to this formula; still we are deficient in analyses of the poorer copper ores of this species, which are so commonly subjected to metallurgical operations, and contain very varying, and often extremely small per-centages of copper, although the ores themselves may at the same time be perfectly free from gangue, and the only difference is that they are sometimes a shade lighter in colour, and somewhat harder when broken by the hammer. In these cases, therefore, it would likewise seem evident that iron must replace the copper, if we admit the above formula to be correct, and their characters would seem likewise to strengthen the opinion I have above expressed of the isomorphism of these two metals in combination with sulphur.

This question as regards the poorer copper ores could only be decided by a greater number of analyses, especially to settle whether the per-centage of sulphur remains constant, as is generally supposed to be the case. It has been assigned as a reason for the fluctuating per-centage of the copper pyrites, that it results from admixture with iron pyrites. If, however, we inspect them minutely, even under a glass, they will generally be found to present a most perfect and homogeneous mass, and apparently of the same structure throughout, and they do not at all present the appearance of a mixture of two different minerals, which we certainly in this case would expect, were iron pyrites mechanically distributed through the mass.

In conclusion, although, from the imperfect nature of the present communication, no definite results can be arrived at, still, by drawing attention to these facts, I would express a hope that it may lead other chemists to examine the matter more thoroughly, and communicate their conclusions, which could not fail to be acceptable both to the chemist and mineralogist.

On the Physical Geography of the Alps.

Messrs Hermann and Adolph Schlagintweit, two young but intelligent and accomplished Bavarian naturalists, who, during long residences in the Alps, particularly in the Eastern Alps (Carinthia, the Tyrol, &c.), in the years 1846–7–8, were actively employed in investigating the numerous and interesting relations illustrative of the Physical Geography of the Alps, have just published a beautiful and remarkable work, entitled 'Untersuchungen Über die Physicalische Geographie der Alpen in ihren beziehungen zu den phœnomenen der Gletscher, zur Geologie, Meteorologie und Pflanzen Geographie.' One volume imperial octavo, pp. 600, with maps, plates, and woodcuts. A rapid perusal of part of this work, of which one copy only has reached Edinburgh, enables us to submit to our readers a few observations on it, and also some illustrative extracts.

This work is divided into four principal sections, consisting of researches, *first*, on the glaciers; *second*, on the geology; *third*, on the meteorology; and *fourth*, on the botanical geography of the Alps.

I.—Glaciers.

The natural history of the glaciers has, since the time of Saussure, engaged the particular attention of naturalists, of whom the most distinguished are Agassiz, Charpentier, Professor J. D. Forbes, and the Schlagintweits. These last-mentioned naturalists enumerate in their work all that was previously known in regard to glaciers, and many interesting circumstances not noticed by others. We have only leisure at present to remark in regard to glaciers, that the most important discussion refers to the nature of their motion. Our authors consider it due to the movement of the *granules of ice*, which they say form the chief bulk of the glacier. This explanation appears to us to be the theory of Professor J. D. Forbes, our authors substituting coarse granules of ice for the theoretical molecules of the viscous mass. But the fact announced by M. Person at p. 172 of the present volume of this Journal should be explained more fully, and formally brought into play in discussing this curious subject.

Section II.—Geology of the Alps.

1. *Origin of Valleys.*

In section second, which treats of the geology of the Alps, we have many interesting remarks by Mr Adolph Schlagintweit on the formation of valleys and the forms of the mountain-chains and mountain-groups of the Alps. Valleys it is remarked by him have been sometimes regarded as almost exclusively the effect of violent floods and torrents, but in later times causes more complicated and connected with the geology of the district have been sought for. Bouguet and Buffon believed that in most valleys the salient angles of one side corresponded with the re-entrant angles of the opposite declivity of the valley, and that all valleys have their origin in the serpentine windings of submarine currents ; whilst by Pallas and Saussure, diluvial floods and erosion by streams and by atmospheric precipitations, were regarded as partial causes of the formation of valleys.* A local influence was also ascribed to a partial overturning and breaking up of the strata.† We must regard, he remarks, as erroneous the opinions, that the manifold forms of valleys can be comprised in one point of view, and that reduced, with few modifications, to one cause. One easily understands how the great valleys excavated by rivers continually eroding deposits, more or less soft and destructible, are distinguishable from the ramifying valleys of districts, which sometimes are widened out into basins and sometimes contracted into narrow ravines. In the latter, mountain masses of ever-varying profile rise on both sides many thousand feet high, whilst in the former case, above the slopes on either side we meet with nearly horizontal plateaux, but slightly raised above the valleys.

To remarks of this kind follow a series of descriptions of basin-shaped hollows, transverse valleys, longitudinal valleys; these are followed by an account of mountain groups, mountain ranges, and mountain summits. Having travelled much in mountainous countries we think we understand the descriptions of our author, but to those who have not enjoyed such opportunities illustrative maps and plans are absolutely required for the accurate comprehension of an Alpine country. Supposing the descriptions to be well understood without plans or maps, the following observations of Mr Schlagintweit on the cause of the present forms of the valleys and mountain groups and mountain ranges will prove interesting to the reader.

"In treating of the *Causes of the present forms of the Valleys and Mountain-chains*, it is stated, that both erosion by means of rivers and the disintegrating effects of the atmosphere and its precipitations, can be considered as having only subordinate influence on the formation of the Alpine districts. How (it is asked) is it possible for erosion to have effected such equal declivities, not only of the valleys, but

* Compare Voigt on the Formation of Valleys, 1791.

† D'Aubisson, Traité de Geognosie, 1. 1819.

also of the mountain ranges, and such a frequent regularity in the distribution of elevations? How could it be possible for an Alpine valley to be excavated by such means from the summit of Mont Blanc, down to a depth of 3000 Fr. feet (French)?

With regard to the sudden expansion of the basins, characterising the transverse valleys of the Alps, it is stated (p. 200), that this could not have been the result of violent outbursts of water; it not being possible for water to have collected in great masses where no dam was present to restrain it; and, if a dam had once existed, it could only have been cut through by a deep gap, and not removed entirely, without a trace being left through its whole extent. And at p. 207, in considering how far great local collections of water may have been concerned in the formation of these valleys, the author observes, that were their figure due to this form of aqueous agency, they must be regarded as cauldron-shaped cavities, that gradually become filled with debris, and now offer levelled surfaces. But this view is decidedly opposed not only by the fact of the very frequent protrusion of the underlying rock, but by the usual occurrence of the rock surface at the slight depth of 10–12 feet (French) beneath the superficial gravel. That the basin or trough-like forms especially, that is, the retreating of the sides of the valley on both sides, cannot have been effected by the presence of a lake, is sufficiently clear. We would otherwise confound the effect with the cause. The question, whether collections of water have generally occupied these cavities, is easily answered in most cases. We usually find here smaller gravel beds, that by their equal distribution are decidedly shewn to have been deposited in standing waters. The inequalities of the valley-bottoms may, indeed, in many cases be sufficient cause for this; but sometimes (for example, near Lengenfield) the form of the ravine immediately following, and the depth of the erosion of the river channel, shew that here a stopping of the water-course had taken place. At all events, this kind of aqueous operation was only subordinate, and more deeply lying causes for the forms of these cavities must be sought for in the configuration of the whole district, and in the original mode of the formation of the valleys. This is the more evident when we consider that many such basins are separated by precipitous depressions only, similar to terrace-like declivities, where there has been a perfect absence of any dams for the collection or restraint of water.

In the longitudinal valleys, on reaching which the Alpine streams have already lost much of their force, considerable beds of gravel occur (p. 212), which have been cut through by the rivers. Here again, river erosion always appears of slight importance in relation to the extent of these valleys. At the terminal gap-like openings of these valleys the eroding power of the streams is abundantly perceptible; but we can scarcely dare to attribute the cutting through of these rocks to such a cause. The signs of erosion reach at the highest to some 100 feet (French), whilst the rock walls are many thousand feet high. It is remarked (p. 219) that the distinguished observers, L. von Buch, F. Hoffmann, O. d'Halloy, E. de Beaumont, Thurmann, B. Studer, and others, have indeed proved in different regions of the earth, that the formation of valleys is not effected by casual erosion, but is most intimately connected with the causes that gave rise to the general configuration of a district. In relation to this are especially to be regarded the manifold windings of valleys, the great change in their direction and extent; whereas in mere erosion, water would have taken the shortest and straightest passage. It frequently happens also, that a valley cuts through a mountain crest; whilst on the other hand, running water would have taken an easier, and frequently already opened, course to one side. Hoffmann has proved this particularly by the well known Porta Westphalica in the Weserthal; Omalins d'Halloy cites very similar phenomena in the course of the Rhone.

The author considers, therefore, that although running water and atmospheric influences effect important changes on the earth's surface, yet these operations have not been sufficient to give rise to the extensive series of Alpine valleys.

The real causes of the origin of these valleys appear to lie in a series of successive elevations, associated with certain sinkings. The great basins found at the extremities of the valleys and in their wider developments, and repeated on a smaller scale on the declivities of the mountains, seem especially to point to a contraction and expansion (zurück weichen) of the masses. We ought here to observe, that the study of the valley-formation of the Alps can only be well followed out, in proportion as the general position of the strata approaches the perpendicular. For the strata often preserve over large tracts the same direction and dip, and are frequently cut through by a series of valleys without suffering any change. One might expect that in the great basin-like depressions, the inclination of the strata would be in some degree altered. Still we must consider that the uprise or tilting of the strata is unusually steep in the Alps: a partial withdrawal therefore, may happen without any very striking disturbance of the inclination and the succession of strata, and is far more possible than under the conditions of horizontal stratification. Sometimes only are we led to notice very striking disturbance of the stratification, particularly in the limestone Alps, and these indeed, where the greatest irregularity of the valley bottoms has been effected by the deep depressions that even yet are occupied by the Alpine lakes. These are confined chiefly to the north and south districts, and are altogether wanting in the central parts where the crystalline slates abound and where the elevation is most regular.*"

2. *Origin and Formation of Springs.*

Mr A. Schlagintweit, in this section of the work, gives a detailed and interesting account of his investigations and observations on the *Origin and Formation of the Springs in the Alps.* The general results are given thus:—1. In employing springs for the determination of the local temperatures of the earth, it is indispensable that, in arriving at comparable results, we direct our attention to the geological formations and local conditions, on which the nature of the origin of springs is necessarily dependent. 2. The origin of springs is not only connected with the mode of stratification, but also, and that most intimately, with the general character of the rock formation. 3. The fissures and porosity of limestone give rise to important differences in the conditions attendant on this rock and on crystalline slates. In limestone, the springs are seldom, but copious, and, coming through

* Journal of the Geological Society, February 1851.

this rock from other higher districts, often issue with a much lower temperature than is usually found in springs flowing out at such a level. 4. The altitude at which the last springs can occur depends on the general elevation of the mountain mass; their distance from the mean altitude of the summits and crests is greater in limestone ranges than in those of crystalline slates of equal height. In Alpine ranges of similar geological formations this distance becomes far greater, where they rise far above 9000 feet (French), where, owing to the formation of steep precipices and summits, and of massive snow-beds and glàciers, the depression of the limit of springs is, comparatively, very considerable. 5. The diminution of the temperature, in proportion to the altitude, does not take place in an equal arithmetical or geometrical progression. In the valleys it progresses more slowly than at the declivities and summits; and *cæteris paribus* advances more rapidly at higher elevations. 6. Almost the same temperature is found at the limits of the growth of trees in the different Alpine ranges, although the altitude of this limit may itself somewhat vary. We may take 3·5° C. as the mean temperature. Immediately above the limit of arboreal growth, we remark the most sudden diminution of the ground temperature, and the most marked differences between the various springs. 7. The springs in valleys are, at equal heights, warmer than those on the declivities and summits, and this is strikingly perceptible in the higher regions. In like manner, owing to the greater radiation from isolated rock masses, a remarkable depression of the ground temperature takes place in the limestone Alps, on the free declivities towards the north. 8. The minimum temperature of the highest springs in the Alps appears to be 0·8° C. 9. The height of the mountain ranges has considerable influence on the ground temperature. We find at equal altitudes above the sea level the warmer springs where the mean elevation is greater; the isogeothermal lines are thereby subjected to curvatures analogous to those of the lines of elevation in the district. These curvatures are shown in a diagram representing a section of the Alps. The numerical results of the numerous observations (given in a tabulated form at pp. 269–273) made with reference to the temperature of the Alpine springs and the isogeothermal conditions of these mountains.

3. *On the Erosion and Weathering of the Surface of the Earth.*

After considering the hydrography of the Alps; in chapter xii., our authors discuss two important topics, namely, the *erosion* and the *weathering* of the surface of the earth, by natural agencies. The present eroding effects of rivers, and their relation to the formation of valleys in general, are considered. The sudden emptying of large reservoirs of water; notices of the course of the great flood (from the bursting of the Vernagt Lake) in the year 1848, with a table, showing the remarkable differences between the time required for the passage through the valley of the great bulk of the flood-water from place to place, and the usual rate of the river-water passing the same places; the influence of valley-basins on the course of great bodies of water (as seen in the table above referred to); the transport of blocks and shingles; and the collecting of the water of the Alpine lakes.

The subject of *weathering*, or the mechanical disintegration and chemical decomposition of rocks by means of the atmosphere, and its precipitations, succeeds, and demands notices of the physical properties of the earth and its composition, of the nature and properties of humus, and of the influence of vegetation on the formation of the earth; of the influence of glaciers on the destruction of rocky materials, the formation of sand, the transport of shingle, land and mountain slips, and the movement of great masses of debris.

The results arrived at from the study of these and other numerous, allied, and subordinate subjects, connected with aqueous and atmospheric erosive agencies, appear to be :— 1. The influence of the masses of "firn" and glaciers on the Alpine streams is not confined to the increase, but extends also to the various distribution of the water. 2. At a certain depth all of the larger lakes have a nearly constant temperatures, connected with the maximum density of the water; the vertical distance of this stratum from the surface varies according to the mass of the water, the form of the lake basin, and the season of the year. 3. The velocity of the mountain streams, in comparison with the rivers of the plains, is not in the same mass greater than their inclination, while their mass is considerably less. 4. A maximum velocity in the regular course of many rivers in transverse valleys is frequently between 7 and 11 Paris feet per second. Their velocity, however, is at other places so considerable, that they have always force sufficient to move small shingle. 5. The quantity of matter held in suspension in glacier-brooks and all Alpine streams is usually very great, and exceedingly increases their eroding power. 6. By erosive action the bed of a river may be very deeply excavated in the hard rock: such channels reach their utmost development in the more inclined ravines; they remain, however, confined to the valley-bottom, and have no important influence on the formation of the ravine itself. 7. The sudden evacuation of vast reservoirs of water participates very considerably in the phenomena of erosion and transport of rocks. Owing to the velocity and power of these floods, it results that the volume of rushing water is far surpassed by the mass of rocky material washed down and deposited about at different places. 8. The formation of earthy detrital matter (Erdkrume) by mechanical disintegration and chemical decomposition of rocks, proceeds rapidly at the highest summits. Its accumulation, however, and the covering up of neighbouring flat areas, are prevented by the steep declivities and the isolated situation of such points. 9. Vegetation is always highly essential for the fixing of earthy matter on the inclined sides of mountains; hence, at great heights, and in the absence of the growth of grass, the occurrence of humus, even in slightly inclined spots, is but very occasional and isolated. 10. In the Alps, particularly in the case of the crystalline slate rocks, the composition of the earth and its physical properties are very favourable to vegetation. Its proportion of humus is very considerable, even at great altitudes. 11. The glaciers not only aid in producing superficial changes by the transport of their moraine-masses, but also by giving rise to an immense quantity of fine sand, which can usually be carried far away by the rivers. And, lastly, 12. The loosening of great masses of rock by the weather and water cause vast land-and-mountain-slips; and the streams traversing the bottoms of the longitudinal valleys, owing to these fan-shaped, wide-spread masses of rubbish, are subject to frequent and considerable variations in their course.

SECTION III.—*Meteorology of the Alps.*

The third section of the work, which we cannot afford space to notice particularly, treats elaborately of the meteorology of the Alps. It consists of numerous observations and experiments on the temperature of the atmosphere, also its pressure, moisture, prevailing winds, and composition. They found the proportions of oxygen and azote the same at all heights; and that the quantity of carbonic acid increases in quantity up to the height of 10,300 feet above the level of the sea.

The following "*Observations on the distribution of temperature in the Alps*" will interest our readers:—

1. The greatest irregularities, and most considerable local inflexions of isothermals, are observed at the lower heights.

2. Generally speaking, the depression of temperature is also very sensible at stations in the Alps, when proceeding from south to north; and the eastern parts are colder than the western. One degree of latitude produces in the plain of Lombardy a difference of temperature amounting to 0°·7 Cent. In the interior parts of the Alps the difference is 0°·5 to 0°·6 C.

3. If we consider the isothermal lines on a longitudinal profile of the Alps, we find that their forms show some connection with the mean elevation of the different parts of the Alps. The isothermals rise where the mean elevation is greater; they sink at the borders and on smaller groups. This convexity of the isothermal lines in the centre of the Alps is still more considerable if we represent by them the temperature of the earth, since the latter is still more intimately connected with the mass of the mountains, with the insulation and radiation of the rocky substance: whilst for the temperature of the air, differences of that kind are more eliminated by its mobility.

4. The vertical distances of two isothermals are the greatest near the base of the Alps, attain afterwards a minimum, and become in the higher parts again a little greater. The position of the minimum takes place in the northern Alps, and the group of the St Gothand at nearly 6000 French feet; in

the central parts at nearly 7000; for the group of Mont Blanc it seems to be at a still greater elevation.

5. The height corresponding to a depression of temperature of 1° C. is in the mean 540 F. feet = 90 toises = 166 metres, if we compare the lowest stations of *continued observation* with the highest stations; but if we consider the temperature of the highest *summits*, the depression becomes a little greater (510 feet in the central Alps).

6. The mean temperature of the air seems to be, for the highest summits from – 13° to – 15° C.

7. At the greater elevations the temperature of single months is generally altered in this manner:—the temperatures of February and January, of August and July, differ less from each other than they do at lower stations.

8. The influence of the general form of the surface on the temperature is particularly evident when we consider the mean temperature of the months. The valleys during the winter are in general colder than the mountains, the cold air sinking down, and being accumulated in them; during the summer they are comparatively warmer, the heat being reflected and radiated by the insulated masses near them, and circulation produced, especially in the horizontal direction; their climate is therefore subject to greater extremes, though in the annual mean it scarcely differs from the Alps in general. The declivities during the winter are comparatively warmer, since the air near the surface, after sinking down in the valleys, is replaced readily by less cold strata. During the summer, particularly in southern exposures, and if the relative height above the bottom of the valley is not great, they are also a little warmer, since then they can be partially reached by the ascending current of air. But this increase of temperature being smaller than that of winter, these situations have a more constant climate than the valleys. The mean temperature of the year on declivities, particularly with southern exposures, is therefore a little higher than the mean of the Alps in general.

9. Summits and declivities, with exposure to north and to north-east, show also the character of a constant climate; but the temperature of summer is much lower, and consequently the annual mean is also sensibly depressed.

10. The depression of temperature with elevation is greater in summer than in winter, amounting, for example, to 1° C. for 440 feet in July, and for 710 feet in January. The cause of this is, that in the lower parts of the mountains the differences between single months are greater than in the higher parts.

11. The elevation of the point, near which, on a vertical line, the depression of temperature is the greatest, is a different one in every month. It is the highest, when the Alps are covered with snow, in December and January; from March to September this point is generally found near the limit of snow; in October and November it lies below the snow line.

12. The height of the snow line in the months does not coincide always with the same isothermal. In January the snow line and the isothermal of 0° are both nearly on the base of the Alps; but from this time to July and August, the isothermal of zero moves quicker upwards than the snow line, and from August to January quicker downwards. The snow line, therefore, in the first period coincides with isothermals warmer than 0° C.; in July it is even at × 5° C. The snow line, in the usual sense, that is to say, its highest limits in summer, is, at the mean temperature of the year, – 4° C.

13. Over large masses of snow and glaciers there is remarked, particularly on fine days, a descending current of air (glacier wind), which has a great influence on the general depression of temperature near the limits of snow.

14. The absolute extremes of cold on single days are at the lower stations sometimes so great, that they are comparatively but little surpassed by those on the higher points. But the differences between the higher and lower part are much greater if we consider the maxima of heat. The absolute maxima seem scarcely ever to exceed 5° or 6° C. on the highest summits of the Alps. On all days the decrease of temperature is greater at the time of the maximum than at the minimum.

15. Compared therefore to the temperature of high latitudes, the *summits* of the Alps correspond nearly to 70° N. lat. But the climate of the highest elevations on the Alps

is much less severe than that of northern Asia, and is more constant than that of Polar America. Their minima of winter are much surpassed by nearly all stations in northern latitudes; but the maxima of summer are colder than those of nearly all points on high latitudes at little elevation above the sea.

SECTION IV.—*Botanical Geography of the Alps.*

The fourth and last part of the work treats in an interesting manner on the botanical geography of the Alps in the following order:—1. The limits of vegetation according to elevation; 2. The periodical phenomena of vegetation; 3. The influence of vegetation on the thickness of the annual rings of the coniferæ; and, 4. A particular account of the vegetation of the upper valley of the Möll.

We cannot venture to notice more particularly this valuable part of the work, but conclude our very imperfect notice with the following extract on the highest limits of animal life in the Alps:—

"The occurrence of animals in the higher parts of the mountains is connected in various ways with the vegetation which affords food or shelter to them. This is very clearly seen in examining the heights to which the cow, sheep, or goat attain. Their limits generally coincide with those of the Alpine pastures, a regular coating of grass. Cattle, however, seldom go up to the extreme boundary, since they are unable to climb so great a height, where the slopes are also steeper; and more extensive pastures are necessary to render dairy farming profitable. We may take 6500 feet as a tolerably universal limit for cows in the central Alps, while the sheep-Alps reach 7000 or 7200 feet, and the highest regular meadow-like surfaces about 7800. But isolated patches of turf are sought out by sheep and goats far higher, and the traveller is sometimes surprised by meeting little flocks of these agile creatures at elevations of 8500 or even 9000 feet. The chamois and wild goat, which occur but rarely in the Monte Rosa group, ascend to very considerable heights; but traces of the former are seldom seen above 10,500 feet. These animals climb highest in the passes connecting *firn*-basins and valleys. Foxes often reach to a height of 10,000, but the ptarmigan goes higher. Bears also show themselves, though rarely, in the upper parts of the Alps. They are dangerous to the Alpine herds and the chamois. Among the Rodents may be mentioned *Hypudæus nivalis*, discovered by Martins, on the Faulhorn, at 8250 feet. The winter abodes of the marmot also occur still above 8000 feet.

"The larger birds of prey, the eagle and vulture, rise above the highest peaks; even small birds often attain considerable heights. The authors observed a group, among which *Sylvia cyanecula* was conspicuous, near the Wildspitz, at 11,000 feet, but these seem to have been driven up by a violent wind. They often met with small birds, such as the *Fringilla nivalis*, *Accentor alpinus*, on the *firn* masses of the Pasterze, at elevations of 10,000 to 11,000 feet; these were in search of insects, which so often abound on the surface of the *firn*. Zumstein observed the Pyrrhocorax on the summit of Mont Rosa, and Saussure observed the same bird on the Col du Geant at a height of 10,578 Fr. ft.

"Insects seem to be the animals that live at the greatest elevation throughout the year. But in addition to those living high up, many others are found upon the glaciers and *firn* masses, especially *Neuroptera*, which are either carried up by the ascending currents of air, and by winds, or fly towards the bright light of the snow fields, and at length fall exhausted to the ground. The number of dead insects which are sometimes found over the whole area of an extensive plain of frozen snow is astonishingly great. It appears that the insects dwelling highest up are without wings, even the beetles, which indeed tends to their preservation, since they are thus incapacitated from wandering too far from their abodes,—little crevices of the rock, and the last heaps of humus near the mosses and lichens. Heer found five kinds of spiders at 10,000 feet, and eight different spiders, and thirteen beetles, from 8500 to 9000 feet, in the Grisons. It may be mentioned as characteristic of these little creatures, that they are all inhabitants of holes, and usually carnivorous, not living on plants. They are almost all of dark or black colour, even when the same or closely allied species exhibit brilliant colours in lower stations." The infusoria of atmospheric dust and the famous red snow have no definite upper limit.

Upon the Identity of the Marks of Glacial Action on the Rocks in the Environs of Edinburgh, with those observed by the Author on the Continent of Europe and in Spitzbergen. By CHARLES MARTINS, M.D., Secretary of the Geological Society of France, &c., &c. Communicated by the Author.

I have no intention of describing in detail the traces which these glaciers have left behind them in the neighbourhood of Edinburgh, this task having been already accomplished in the most satisfactory manner by Sir James Hall, Professors Agassiz and Buckland, Mr Charles Maclaren, and Mr Robert Chambers. These traces are, 1*st*, Those of mountain-rocks, polished and striated, and identical with those which are seen beneath and upon the flanks of existing glaciers, and in all the Swiss valleys, which have been the seats of ancient glaciers; 2*d*, A clay called till or boulder-clay, more or less pure, filled with boulders, angular or irregularly rounded, striated in all directions, resembling those which are so universally found under existing glaciers; 3*d*, Angular blocks derived from the northern mountains. Compared with the basin of Switzerland or with the plain of the Po, the lowlands of Scotland are destitute of true moraines, and of that thick aqueous diluvium upon which they repose. But for these differences the analogy with the Vosges, Switzerland, and the Pyrenees is complete; it is still more so if we compare the phenomena of the erratics of Scotland with those of Scandinavia. At the same time, I may remark, that I have not observed, in those parts of the country I have visited, those hillocks of sand covered with erratics which are analogous to the *Osars* of Sweden.

At the last meeting of the British Association held in Edinburgh in August 1850, the Geological Section, upon the suggestion of its president, Sir Roderick Murchison, devoted an entire sitting to the discussion of the causes of these interesting phenomena. Two systems were brought under review; the former ascribing the above phenomena to the action of floating icebergs, which, influenced by powerful marine currents, had rounded and striated the rocks *in situ*, had striated the boulders and pebbles, and transported the blocks; the latter admitted the direct action of the glaciers which, at an early period, had pervaded Scotland, as they now cover Spitzbergen and the northern parts of Baffin's Bay. The old hypothesis which accounted for the phenomena from the agency of mighty currents found no supporters. It is true that in Scotland it would be still less defensible than in the Alps, or the Vosges, since the lower aqueous diluvium of the glacier regions is there altogether wanting.

The discussion of the two hypotheses, that of glaciers, or of floating ice, will be the easier, as I have only to repeat the admirable arguments adduced in favour of each, by the president, and by the various intelligent members of the Geological Section who joined in the animated discussion. The circumstance of my having twice visited the western coasts of Spitzbergen enables me to produce some additional arguments to those which were so ably brought forward.

I may at once premise, and with the greatest satisfaction, that the ancient existence of glaciers in the mountains of Scotland was implicitly admitted by both parties, and, in fact, if floating icebergs have striated the rocks in the neighbourhood of Edinburgh, these icebergs proceeded from glaciers, and these glaciers existed in mountains situated to the north in Scotland: for the transported blocks did not come from Scandinavia, nor from Iceland, nor from Greenland, they were all aboriginal, *autochthones*. To attribute another origin than that of glaciers to floating icebergs, *charged with blocks*, would be to contradict all the facts witnessed by the navigators who have traversed the glacial seas of both the poles. But, to admit of glaciers in the mountains of Scotland, is to admit the glacial period, and the ancient extension of glaciers in Scandinavia and Switzerland, in the Vosges and the Pyrenees. Hence we may venture to state that the Geological Section of the British Association has given its judgment in favour of the idea which the genius of Playfair had conceived, and which has since been brought out by the labours of Messrs Venetz, Charpentier, Agassiz, and Forbes. The question, then, which we have now to examine, is a secondary one. Every one admits that at a certain epoch the high valleys of Scotland were occupied by permanent glaciers. According to the one party, these glaciers descended to the shore of the then existing sea, and covered the Scottish lowlands, and were in that condition in which we now find those of Spitzbergen at the present day.* According to the other party, the portion of Scotland which is comprised between the Frith of Forth and the mouth of the Clyde was submerged under the waters of the sea, and the icebergs detached from the glaciers of the mountains floated upon the sea, and were borne by a current which flowed from west to east.

Examination of the Theory of Floating Icebergs.

It is indisputable that the relative levels of the land and sea have undergone a change in a great portion of the coast of Scotland. The terraces which bound the shore, and the mollusca which are still living in the north sea, and whose shells are still found at an elevation considerably above the highest tides, incontestibly prove this. Messrs Prestwick and Craig have pointed out those of the number which belong to living species, reaching to an elevation of 350 feet, at Gamrie and Airdrie, near Glasgow.† What, however, appears to me in no degree demonstrated is, that this immersion actually occurred during the glacial epoch. The direct contrary would seem very much more probable, since the shells of Gamrie are of the existing species, and are still found in the neighbouring sea. But for the support of the hypothesis of floating icebergs it is necessary to prove that the whole of the flat land had been plunged under the water during the *cold period* to a considerable depth: in fact, the polished rock situated upon the southern side of Arthur Seat is not less than 400 feet above the level of the sea. There is one on

* See my Memoir upon the Glaciers of Spitzbergen. Edinburgh New Phil. Jour., vol. xxx., p. 284. 1841.

† Proceedings of the Geological Society of London, 1837, p. 549. And Mr Smith, Jordanhill, Mem. Wernerian Society, vol. viii. The shells of Gamrie are Astarte scotica, Tellina tenuis, Buccinum undatum, Natica glaucina, Fusus turricola, Dentalium dentalis, &c.

the north limb of the hill, 500 above the sea; whilst, on the Pentland Hills, there are, near Dunsire, striated rocks at the height of 800 or 900 feet, and on the east of Cairnhill at not less than 1400 feet.* But what proof have we that, since the glacial epoch, the middle region of Scotland has been elevated 1800 feet? In Norway, that classic land of change of level, the shelly bed—*skalen-schicht*—does not exceed, according to M. Keilhau, 565 feet,† and the most elevated terraces reach only to 600 feet.

The theory of floating ice, then, must necessarily be supported upon a *previous hypothesis*, namely, that at the epoch of floating icebergs the mean level of Scotland was at least 1400 feet lower than it is at present; or, in other words, that it had undergone a change of level three times greater than that of Scandinavia.

Let us now then study the mode of the formation of floating icebergs, and the action which they can exercise upon the rocks *in situ*, when they are impelled by marine currents. All being agreed that these floating ice-masses must be assimilated to those which now still detach themselves from the polar glaciers, we shall, in a few words, review the mechanism of their formation.

When at Spitzbergen, in summer, a glacier descends in a valley which opens into the sea, it stops not at the shore, but continues to advance, and enters into the ocean. The portion of it which plunges into the water gradually melts; and at low water the inferior part of the glacier rests suspended, so that we can perceive an interval or space between the water and the ice. It is under these circumstances that portions of the glacier are detached, drop into the sea, and are set at liberty by the ebbing tide. Such is the origin of floating icebergs. Before these floating ice-masses can wear and groove the hardest rocks, such as trap, it is necessary that they carry upon their inferior surface, pebbles or grains

* Mr Maclaren on Grooved and Striated Rocks in the Middle Region of Scotland. Edin. New Phil. Journal, vol. xlvii., p. 176. 1849.

† Daubrée. Bulletin Soc. Geolog. de France, vol. xiv., p. 574. 1843.

of sand, which must be encased in the ice as the diamond wherewith the glazier cuts glass is fixed at the end of a handle, or as a precious stone is infixed into the bezel of a ring. Thus is it that the existing glaciers mark the existing rocks upon which they glide. All the travellers who have visited the glaciers of the Aar, of Grindelwald, of Rosenlaui, &c., have been able to collect many pebbles and detached gravel projecting from the ice in which they were enveloped. But the question here occurs, do these pebbles remain imbedded in a floating ice-block when it is detached from a glacier? This has never been demonstrated. And in truth we may affirm, that it is at the inferior surface of the GLACIER,—at that part of it which still reposes upon the sand,—that these pebbles are numerous. We have moreover observed that when this part of it reaches the sea, it melts and disappears, and consequently all the pebbles, and even the large blocks, fall down as fast as the bed in which they are contained passes into the liquid state. Hence, then, I have never perceived pebbles beneath the glaciers of Spitzbergen whilst examining their inferior surface at low tide, when the interval between the water and the ice has been about three feet apart. When, then, a part of the glacier comes to be detached, falls into the sea, and becomes a floating iceberg, *it has ceased to have any pebbles at its inferior surface.* As it respects those at the upper surface, they adhere but slightly, because pressure has not forced them into the ice. But it may be alleged that this mode of the formation of floating icebergs is not the only one,—that it occurs, indeed, at Spitzbergen, where the temperature of the sea is, in summer, somewhat above 32° Fahr., but is not the same everywhere; and the voyagers who have traversed Baffin's Bay in search of the unfortunate Sir John Franklin, have observed in these regions that glaciers entered the sea without being melted thereby, and are carried along its bottom. In these cases, it is maintained that the floating icebergs may carry pebbles incased in their inferior surface. This no doubt is true. But then, not content with the hypothesis that the lowlands of Scotland were submerged in the ocean to the depth of

1400 feet beneath its present level, we have moreover to admit a climate so extremely cold that, even in summer, the temperature of the sea never rose above 32° Fahr., we implicitly admit an amount of cold much more intense than that which suffices for the explanation by the presence of ancient glaciers for the phenomena in the neighbourhood of Edinburgh.

But these objections against the theory of floating icebergs are not the only ones. When one of these enormous fragments is separated, and falls into the ocean, it plunges deep, rotates upon itself, reappears upon the surface of the waters, swings and oscillates many times, till it attains a position of equilibrium, which again is dependant upon that of its centre of gravity. Seven-eighths of the floating mass are submerged, one-eighth swims above. Hence, it rarely happens that the inferior surface remains beneath; and often, in reality it is turned upwards, or upon its sides. Under these circumstances it can neither wear nor striate the rocks over which it passes, for ice has not the power to polish the harder rocks, such as traps and granites. Hence we perceive, that a very small number of floating icebergs are in the necessary conditions to striate the rocks,—namely, having their inferior surface turned downwards, and containing imbedded pebbles. In connection with this, it is not to be forgotten that the *roches moutonnée* or rounded eminences so common in the environs of Edinburgh, cannot have been produced by casual icebergs.

We have hitherto all along supposed, with the partizans of the theory we are combating, that a floating iceberg sweeping along with a current, had the power to wear, to polish, and to striate a rock *in situ*. Nevertheless, we find it impossible to conceal our doubts, and not to express our incredulity in this matter. The existing glaciers produce these effects under our eyes, because they act upon the underlying rocks like a colossal pressing-machine. Confined by the flanks of a valley they descend slowly, and very much in the same direction: their prodigious weight, varying from that of 200 to 1200 feet of ice, presses upon the rock, and wears and striates it by means of the sand and gravel which is interposed ;* and hence it is very clear that the hardest rocks cannot resist a friction so completely overwhelming. But on the other hand, when one has witnessed the floating icebergs, the sport of the winds and waves, dancing upon the surface of the sea,—when we have seen them stranded on the shore, and rocking on themselves,—when we have noticed them arrested by a trifling obstruction, we can scarcely believe that they ever possessed a power to wear away and striate the rocks. It is true, however, that recourse is had to the agency of currents which are endowed with an extraordinary velocity ; while, in a state of matters so nearly approaching to these now existing, and which still occur in the Polar Seas, we cannot see how any one can venture to contend for currents more rapid than those which now prevail. As it regards the ocean, I am acquainted with only two facts which bear on the action of floating icebergs ; the one reported by M. Forchammer, who witnessed a very deep furrow traced on the humid argillaceous sand upon the coast of Denmark. The fishermen declared that this furrow had been produced by a block which had been wafted by the ice that had been produced on the margin of the ocean.† The other is that of striæ produced on the rocks in the Bay of Fundy, Nova Scotia, at the foot of Cape Blomidon, situated at the bottom of this gulf.‡ Sir Charles Lyell perceived upon some banks of *soft sand-stone*, washed by the waves, two groups of *striæ* within the distance of a quarter of a mile. The striæ were rectilinear, and formed very small angles with each other, which were parallel to the direction of the shore. The peasant who acted as guide to Sir Charles, *being interrogated as to the origin of these marks*, answered, " that in the preceding winter, that of 1841, he had seen packs of ice borne away by the tide with a velocity of ten miles an hour, so forming a continuous mass from one side to the other. At the foot of Cape Blomidon they were fifteen feet thick, and

* See my Memoir, Edin. New Phil. Journ., No. 43, p. 54. 1847.

† Bulletin de la Soc. Geolog. de France, II. Livre, No. iv., p. 1181. 1847.

‡ Travels in North America, vol. ii., p. 173. 1849.

were forced along the shore: he moreover imagined that the boulders of trap which fell from the beach, are often impacted into the ice and carried away along with it." It happens unfortunately then, that the explanation of the interesting fact pointed out by Sir Charles Lyell, is derived from a peasant who never confirmed it by direct observation, who never had *seen* the fragments of the trap infixed in the ice, any more than he had seen the striation actually effected; whilst at the same time the whole country being covered with striated rocks which reach back as far as the glacier epoch, doubts must necessarily exist concerning the recent origin of those in question. We may add that the striation of a soft sand-stone, by floating ice-blocks, would be much more easily effected than that of hard rocks, such as that of granite, trap, &c. This question then cannot be determined: and hence we shall take the liberty of recommending the subject to the attention of those geologists who inhabit the coasts of the gulf of Bothnia, and especially to the learned professors of the University of Helsingfors.

But to my apprehension there still exists another difficulty. According to the acknowledgment of all competent observers, the polished and striated mountain rocks present the same appearances in the plains of Scotland, in the mountains and in the valleys of Switzerland, and in the immediate neighbourhood of the existing glaciers. I myself possess a collection of polished and striated rocks procured from the Alps of the Jura, from the Vosges, from the Pyrenees, from Scotland, Scandinavia, and from North America, and the whole of them are identical, as compared with each other, and identical with those which I have detached from the very ice of the glaciers themselves. It becomes necessary, then, that the partizans of the theory of floating icebergs proceed to demonstrate by *direct observations*. 1*st*, That the floating ice-blocks proceeding from marine glaciers can striate the hardest rocks. And, 2*d*, That the striæ which they produce are *in every particular identical* with those which the existing glaciers effect. Unless I am very much deceived, I am persuaded that, ere the first proposition be established, it will be found that a difference exists between the traces left by a glacier slowly descending in a valley, and those effected by floating icebergs carried along with a current, but at the same time propelled by the winds, and buffeted by the waves. Forces so exceedingly fortuitous and capricious could scarcely ever produce that parallelism of striation which has been ascertained to exist all around the Frith of Forth.

Striated Pebbles.—In examining the boulder-clay of Ross-shire, Mr Hugh Miller observed that this deposit rested upon polished and striated rocks, and that the pebbles contained in the clay are, like those in the neighbourhood of Edinburgh, covered with streaks. These streaks appeared to him to be generally longitudinal, that is to say, parallel to the great axis of the pebbles. Of this phenomena Mr Miller has found an explanation which has a reference to the striations both of the rocks *in situ* and of the pebbles. Let us suppose, says he, a great raft of wood descending a river, if it grazes heavily upon a sunken bank covered with pebbles, reposing upon a rock, it will propel them along without causing them to roll, and then they will leave these striated marks upon the rock *in situ*, and, in turn, will themselves be striated—they will, according to a Scottish expression, be both *scratchers* and *scratched*. Put now a floating iceberg in the place of a high raft of wood, and you have the explanation of the striated pebbles of the boulder-clay of Scotland.*

Notwithstanding the ingenious comparison of Mr Miller, I cannot accept of his explanation. For I do not believe that pebbles which are not incrusted in the ice could striate rocks, and be in turn striated themselves. If that were the case, we should find these striæ in the bed of the impetuous torrent of the Alps, or marks of pebbles upon the blocks so often carried along at so rapid a rate. But never in these torrents has any one shewed that the rock which forms its bed, or that the pebbles therein contained, presented the appearance of *striæ*. They are polished, but never striated. This is a fact which I have certified in the Alps, and also on the sea-shore, where a striated pebble has never been

* See the Scotsman newspaper, August 3, 1850.

picked up by any observer. The action of the waves polishes them, but never striates them. But, besides all this, if, at the foot of the glacier of Grindelwald, you examine the pebbles which form its terminal *moraine*, you will find that they are all striated. These same pebbles, however, ere long, fall into the torrent which escapes from the glacier, where, rolling one upon another, they speedily lose all their striæ. Hence it becomes impossible to find in the torrent a single striated pebble at the distance of two hundred fathoms from the glacier. My friend, Mons. Ed. Colomb, has made observations precisely similar in the torrent of the Thur, which runs through the ancient moraine of Wesserling in the Vosges, which is very much composed of striated pebbles. On one occasion he offered a reward to any workman in the manufactory who would bring him a striated pebble procured from the torrent at the distance of two hundred fathoms from the moraine ; but the reward was never challenged. Supposing, then, with Mr Miller, that the streaked pebbles were marked by the passage of a floating ice-block, these pebbles having been subsequently carried away, and rolled by the current *of no great depth*, which he invokes, and buffeted by the waves, all the striæ would speedily have been effaced, and the boulder-clay would not have contained a single streaked pebble.

As to the predominance of the longitudinal over the transverse striæ, it exists to a certain extent in the striated boulders or pebbles of Scotland, as in those of the existing glaciers; and it is the necessary consequence of this mode of formation. In fact, all the pebbles which are placed between the glacier and the overlying rock are by it drawn along in its insensible progression. Compressed between the ice and the rock, they are rudely fretted, worn, and striated ; whilst, at the same time, it is plain that the pebbles will have a natural tendency so to arrange themselves that their longitudinal axis will be in the direction of the progression of the glacier. Hence, then, the predominance of the longitudinal striæ. At the same time, I would remark that I do not attach any considerable importance to what Mr Miller has said concerning the direction of striæ upon the pebbles. Even the mechanism of their formation clearly shews that they must accommodate themselves at every possible angle, since the position of the pebble, confined between the ice and the rock, is continually changing. Actual observation upon the existing glaciers has often demonstrated this to all those who have studied the phenomena among the Alps.

Proofs of the existence of Ancient Marine Glaciers in the Environs of Edinburgh.

I shall now attempt accurately to demonstrate that all the glacial phenomena in the neighbourhood of Edinburgh may be explained on the hypothesis of the existence of ancient glaciers which descended to the shores of the ocean, and by slight changes of the level of the country since that epoch. For the purpose of demonstrating this truth, I am not obliged to suppose a general immersion of Scotland below the ocean to a depth greater than that which is indicated by the most elevated shell-beds, nor to admit that the floating icebergs streaked the rocks, and that their striæ are identical with those of the existing glaciers. My proposition may be thus expressed—*that at the glacial epoch, Scotland was covered with glaciers, as Spitzbergen is at the present time.* Let us then consider if the facts confirm this comparison and resemblance.

The polished and striated rocks which I have examined at Blackford Hill, at Corstorphine Hill, at St Margaret's works (North British Railway), at the northern foot of Arthur Seat, also at Granton, upon the Pentland Hills, round the Gare Loch, near Glasgow, are all identical with those which the existing ones now fabricating—so to speak—under our eyes; and they differ in no respect from those which I have observed in Scandinavia, in the Alps, and in the Vosgian range. It is then assuredly both simple and natural to attribute them to the action of glaciers, whose existence at this period, in the mountains of Scotland, has been doubted by none. But this is not all. My excellent friend Mr Charles Maclaren, who has contributed so much to the elucidation of this subject, directed my attention, in the neighbourhood of Edinburgh, to a polished and striated rock, the phenomena connected with which cannot otherwise be explained than by the action of a glacier. This remarkable rock occurs at the foot of the southern face of Blackford Hill, on the bank of the streamlet called Braid's Burn. The rock, which is phonolitic, overhangs at an angle of 40°, forming a recess or cave ten feet deep, and the roof or overhanging surface of this recess is polished and striated for a distance of thirty feet. The streaks run from WNW. to ESE., that is, in the direction of the valley, and in many places may be perceived the quartzoic sand which has produced the striæ still adhering to the rock. In Switzerland we have frequent opportunities of demonstrating, on the margins of the glaciers, that the sloping sides are striated *from beneath* by the plastic mass of the glacier, which forces itself under the rock, and polishes its inferior surface ; but how are we to explain a similar fact by the action of floating icebergs ? These not being able to carry pebbles, except at their lower surface, we must needs suppose that Blackford Hill had been striated by floating icebergs, which had been capsized in such a manner that the lower surface had become the superior. It must, moreover, be admitted that these ice-blocks were so situated under this rock, which at this period must have been at the bottom of the ocean, and possibly 1200 feet below its surface. And all this admitted, the difficulty is not even yet removed; for if the imagination conceives the possibility of a floating iceberg grazing the rocks by *pressing on them with all its weight*, it cannot comprehend how this ice striates by its upper surface, or by its side, whilst it passes under a rock conveyed by a current. Mechanical agency and experience alike teach us, that the most trifling obstacle stops or diverts a floating iceberg, and that the impulsion which it receives in a current will never be sufficiently powerful to enable it to streak a rock so hard as the one at Blackford Hill.

The freestones and traps in the neighbourhood of Edinburgh having been striated by glaciers, these must have descended from mountains; and as a matter of fact, the researches of Scottish glacialists have discovered specimens in most of the valleys. Mr Charles Maclaren and Mr Smith of Jordanhill having requested me to visit that of the Gare

Loch, in Dumbartonshire, I have found, as they did, that the valley which ascends from the extremity of the bay to the ridge which separates it from Loch Long, furnishes mountain rocks, polished and striated, as well characterised as those of the most celebrated of the Swiss valleys in this particular. To deny that a glacier occupied that valley would not be more discreet than to deny that of the Aar at Nivele, that of the Handeck, and the Hellenplatte.

In the neighbourhood of Edinburgh the direction of the streaks presents a difficulty which I wish in no degree to depreciate. Upon the Pentland Hills, and in some other localities to the south of the town, the striæ are directed from the west to the east, and at first sight one might be tempted to believe that they should have been from north-west to south-east; in other words, from the mountains towards the plain. In studying the large map of Scotland by Mr Arrowsmith, and comparing it with the direction of the striæ indicated by Messrs Maclaren and R. Chambers, we may explain what they effect to the south of Edinburgh. A great glacier issuing from the groups of Benledi, of Benchochan, and of Benclach, and thence turning eastward from Benlomond, descended first into the valleys of the Forth and the Teith, followed the direction of the Forth, and covered all the country which surrounds the Frith of Forth from Stirling to Edinburgh. Upon both sides of the Frith, the direction of the striæ indicates that of the glacier, which advanced from north-west to south-east, as does the Frith itself. But another glacier, not less powerful, descended, turning westward from Benlomond, from Benunack, Benlochen, Benvaigo, and Benviolay, and filled, as the direction of the striæ will still teach us, the depressions of Loch Lomond, of Loch Long, Loch Goil, and of the Gare Loch; then it spread itself over the plain of which Glasgow occupies nearly the centre. But in this plain it would encounter the great glacier of the Forth, and push it towards the east; and hence this direction from west to east of some of the groups of the striæ to the south of Edinburgh. Existing glaciers follow the course of the valleys; ancient glaciers did the same; and when two glaciers of unequal force encountered each other, the stronger repelled the weaker. This we actually witness in the existing glaciers, and in particular upon that of the Aar, and the Mer de Glace of Chamouni. The plain of Switzerland presents the same anomalies; and it is often very difficult to assign reasons for the direction of the striæ, because it was so overwhelmed with glaciers, which mutually crowded, repelled, and disturbed each other. Thus, then, without attempting to disparage the difficulty resulting from the direction of the striæ discovered to the south of Edinburgh, I believe they will not prove an insuperable objection against the alleged influence of the glaciers. And more; I am convinced that the Scottish geologists, in attentively observing the direction of all the visible striæ between the Friths of the Clyde and of the Forth, and in comparing them with the origin of the transported boulders, will be able exactly to trace upon a map the limits of the glaciers which had invaded the plain, and so will be able to give a complete account of the direction of the striæ which run from west to east.

We have just seen that the rocks of Scotland are covered with striæ identical with those which mark existing glaciers: and we now remark that these striated rocks are covered over by a clay which contains striated pebbles. If you dig beneath one of the glaciers of Switzerland, that of Grindelwald or of Rosenlaui for example, you will find exactly the same thing. Before you discover a polished rock you will be obliged to remove a thick bed of mud and of pebbles, which is the boulder-clay of Scotland in miniature. The striæ which cover these pebbles prove that, from the moment they were striated, they—the pebbles—have not been water-rolled. The clay or till, which is the analogue of the mud of the glacier, and of the *lehm* and *loess* of Swiss geologists, is the result of the complete trituration of rocks by the glaciers.

The environs of Edinburgh, then, present three of the characteristic signs which a glacier leaves behind it,—the polished rocks, the striated pebbles, and the glacier mud. We shall also find transported blocks, though no veritable moraines. In explanation of this apparent anomaly let us recal to recollection the manner in which the moraines are formed from the glaciers. Wheresoever high summits and escarpments prevail, there the blocks, stones, and fragments which are detached, fall upon the ice and form long dikes, the necessary result of the progression of the glacier. But, on the other hand, when a glacier is overtopped only by a small number of peaks, and those of small elevation, then few or no fragments fall upon its surface, and no moraines are formed, either lateral or terminal. Thus, at Spitzbergen, where the mountains are, as it were, buried in the ice, where you see only the most elevated peaks in the centre of the island, the moraines are reduced to very small dimensions. In Scotland, at the time of the greatest extension of the glaciers, it was very much the same ; and I am convinced that very few summits surpassed the peaks of the ice. When, from the hills which bound Gare Loch, we observe the high mountains which divide that loch from Loch Lomond, we are surprised to see that they are rounded and *moutonnée* to their very top. Now, the invasion of the environs of Edinburgh by the glaciers evidently corresponded to the maximum of their extension, in other words, to the epoch when the mountains themselves were covered under a mantle of ice. Let us not be surprised, then, when we do not find true moraines in the neighbourhood of Edinburgh ; but only striated pebbles and erratic boulders at a great distance from each other : this peculiarity finds its explanation in the limited elevation of the mountains in so northern a region. When we consider that at this epoch the Vosges were equally covered over, and the valleys of the Pyrenees were occupied with glaciers, we can understand that Scotland then represented exactly the present existing state of Spitzbergen.

*Zoological Proofs of the Ancient Existence of Marine Glaciers in Scotland.**

Mr Smith of Jordanhill has shewn that, during the epoch of extreme cold, the maritime Fauna of Scot-

---

* Many years ago I received from Mr Smith a collection of the Clyde fossils. I proposed to him sending them to Mr Deshayes of Paris, the celebrated naturalist. This was done, and in due time a named catalogue of them was returned to me, with the intimation that some of the shells were *arctic.*—*Ed. Phil. Jour.*

land was more arctic than it now is. He has thus furnished us with the zoological proof of the ancient existence of glaciers in this country. Immediately beneath the boulder clay, there is found in many localities on the Frith of Clyde a laminated clay which contains shells. Sometimes, as at Paisley, for example, this clay also contains pebbles. Mr Smith, and, after him, Professor Edward Forbes, have recognised that the most part of these shells appertain to a truly arctic Fauna. Mr Smith even assisted me in gathering a certain number of these shells at Helensburgh and Paisley, and has presented me with others collected in different localities. He has also submitted them to the examination of M. Deshayes, who has authenticated nearly all of Mr Smith's determinations, and established the arctic character of this Fauna.* I give this list as an abridged example, and refer the reader for the additional particulars to the complete enumeration of the species which Professor Edward Forbes has inserted in the Memoirs of the Geological Survey of Great Britain, vol. i., p. 406.

The most Common Arctic Shells of the Laminated Clay.

Mya truncata, *L.*, *var.* udevallensis; Saxicava Sulcata, *var.* udevallensis, *Sm.;* Tellina calcarea, *Gm.;* Astarte borealis, *L.;* Astarte Arctica; Cyprina islandica, *L.;* Mytilus umbilicatus; Pecten islandicus, *Muller;* Fissurella noachina; Littorina littorea, *L.;* Littorina castanea; Fusus Scalariformis, *Gould;* Buccinum undatum, *L.;* Natica clausa; Balanus sulcatus, *Lam.*

Let us now observe how the presence of arctic shells in an upper bed of clay, of boulder clay, or even throughout its thickness, may be explained. Let us return, then, to Spitzbergen. Here the glaciers descend to the very verge of the sea, bringing along with them striated pebbles, glacier mud,

---

* These shells must not be confounded with those which are found upon the terraces, or ancient sea-beaches which so much surround the coast of Scotland. These are identical with the present species living in the sea. One may easily verify this statement close by the Granton quarries, where the terrace is 20 feet above the sea. These terraces prove that the coast has been elevated since the termination of the glacial epoch.

and erratic blocks. Having reached the extremities of the gulfs and deep bays which disrupt the coast, as do the friths of Scotland and the *fiords* of Norway, they advance beneath the sea, and let fall to the bottom of the water the angular blocks, the mud, and the striated pebbles. These sink into this glacier mud, and, as the sea is tranquil at the bottom of these bays, they are not rolled by the waves, and maintain their streaked marks, especially if the sea is deep.* But the present seas of Spitzbergen are not inanimate; and there are found numerous mollusca, even in the neighbourhood of the glaciers. On the death of the animal the calcareous envelopes are deposited in the glacier mud which, on the shore, forms the bottom of the sea. Suppose, then, that the coast sinks somewhat, the glacier will recede, and we shall then have precisely what now exists in the lowlands of Scotland, namely, polished and striated rocks, covered with glacier mud, containing striated pebbles and arctic shells.

The elevation of Scotland *since the termination of the glacial epoch* is not a gratuitous hypothesis, since its coast is fringed with sea-beaches covered with shells, which belong to existing species in the neighbouring seas, and the shells of which Mr Prestwich has found near Glasgow at an elevation of 320 feet above the ocean. It is since these observations were repeated by Mr J. Smith, that it has been found that these shell-banks are always placed above the clay and the arctic shells.

In recapitulation, I remark, that the glacier traces in the neighbourhood of Edinburgh appear explicable by the following succession of phenomena. 1*st*. By the ancient existence of glaciers, which descended from the mountains to the sea-shore.† 2*d*. The partial *immersion* of the country during the glacial epoch. 3*d*. The subsequent *emersion* at the close of this epoch. Hence the existence of striated rocks, of

* This depth of the sea, previous to the advance of the glaciers, is not a simple supposition. Near the terminal escarpments of those of Bell Sound and Magdalena Bay, I have found a depth reaching from 100 to 360 feet.

† I am delighted to find that, as it respects the turning point of this explanation, I am supported by the authority of Professors Agassiz and Buckland, and also by Sir Charles Lyell, who stated their views some ten years ago.

glacier mud, of streaked pebbles, of arctic shells, and of raised beaches covered with existing shells.

If this attempt at explanation does not ensure the conviction of the reader, I trust that he will at all events concede that I have been very moderate in the use of hypothesis. I have compared Scotland to a country of an analogous elevation, where the ancient extension of glaciers is completely realised; I have based my explanation upon the striated rocks, the boulder clays, and the striated pebbles—upon the observation of existing glaciers; I have demonstrated the *subsidence* during the glacier epoch, by the arctic shells, and the *emersion* after the termination of the cold epoch, by the raised beaches covered with modern shells. If, then, I have deceived myself, I need fear only as it regards my deductions, for the observed facts which I have invoked have been very often verified by competent observers.

On the Emery of Commerce.

The emery of commerce, properly speaking, is not a simple mineral, but a compound of granular corundum and oxide of iron, in which the former usually predominates. The oxide of iron present is under the form of magnetic oxide, more or less mixed with iron glance; sometimes it is titaniferous. The aspect of the substance differs considerably according to the locality.

The emery of Saxony is of a bluish grey colour, and is intermixed with talc and steatite.

The *Naxos emery* is of a bluish grey, with a mottled surface, with small points of a micaceous mineral disseminated.

The *Gumuch-dagh emery* is commonly of a fine grain, and dark blue, bordering on black, not unlike certain varieties of magnetic iron ores. With this variety we frequently find pieces of corundum of some size. The interior of the mass is tolerably free from the micaceous specks found in that of Naxos.

The *Kulah emery* is usually coarse-grained, and much darker than that of Gumuch-dagh, its external surface resembling sometimes that of chromate of iron.

The *Nicaria emery*, in many instances, presents a slaty or lamellated structure to a very remarkable degree, so much so that certain specimens might pass for gneiss. The colour is dark blue, and somewhat mottled, like that of Naxos. There is also much that is quite compact that is found in the same locality. The lamellated variety contains an abundance of a micaceous mineral, which in this instance appears to have determined its structure.

The *Samos emery*, as yet found only in small quantities, and in the form of nodules, is uniformly of a dark blue colour, sometimes of a coarse-grained, and at other times of a fine-grained structure, not unlike certain varieties of a very compact blue limestone.

Emery, at first sight, might be confounded with several ores of iron; as magnetic iron, certain varieties of iron glance, and sometimes with chromate of iron.

Geographical Distribution.—Prior to the latter part of the year 1846, the period when Mr Lawrence Smith, the American missionary, discovered emery in Asia Minor, emery was supplied to the arts almost entirely from the Island of Naxos, in the Grecian Archipelago; and the proprietors of the mines there controlled completely the price of this substance. The emery from Naxos frequently went under the name of Smyrna emery, from the fact of its coming to us from that port, where it is originally carried from the island for future exportation.

Prior to 1846, the existence of emery in large quantities was not remarked in Asia Minor, or any of the contiguous islands, except that of Samos, which fact is alluded to in Tournefort's travels in the seventeenth century. "In the latter part of 1846," says Mr Smith, "I arrived in Smyrna, and was shown specimens which I recognised as emery that came from a place about twenty miles north of Smyrna; they had been first discovered through the agency of a knife-grinder of the country, who had been in the habit of using it to charge his wheels with. The importance of this circumstance to the Turkish government as well as to the arts (emery being at that time sold at a most exorbitant price) induced me to return to Smyrna in the early part of 1847, for the purpose of examining the supposed locality of this mineral. On this second visit other localities were made known to me, that an English merchant of the name of Healy had succeeded in bringing to light."

"The first locality," says Mr Smith, "towards which I directed my examination was that of Gumuch-dagh, a mountain about twelve miles from the ruins of Ephesus." Since the first discovery, other localities have been ascertained by Mr Smith in Asia Minor, and also in the Island of Nicaria, in the Grecian Archipelago. Emery has been long known at Ochsenkopf, in Saxony.

Geognostical position of Emery.—The emery of Saxony occurs disseminated in talc-slate. In the districts in Asia Minor examined by Mr Smith, it occurs disseminated and in masses many tons in weight in a marble overlying mica slate, gneiss, &c. The emery appears to be of a contemporaneous formation with the marble. From the great extent of country through which this marble emery extends, it is possible we may hear of an emery-marble as a distinct formation.

Commercial consideration of Emery.—The use of emery in the arts is of very ancient date, a fact proved by works on hard stones that could not have been executed except by emery or minerals of that nature. It is very probable that emery from the localities which have been mentioned, was used in former ages by the Greeks and Romans. For example, the locality of Gumuch-dagh is immediately by the ancient Magnesia on the Meandre, and between Ephesus and Tralles, twelve miles from each of these cities, and the same distance from Tyria: in all of these cities the arts flourished, and none more than that of cutting hard stones, if we are allowed to judge from the specimens of their skill in this art that have come down to us.

Nevertheless, the quantity of emery formerly employed was insignificant in comparison to the quantity now required, more particularly within the last twenty years, since the use of plate-glass has been extended. The annual consumption at the present time is about *fifteen hundred tons*.

For various reasons, the island of Naxos furnished for several centuries almost exclusively the emery used in the arts, as much for the facility with which it was obtained as in the uniformity of its quality. The emery exists in very great abundance on this island, and notwithstanding the quantity already extracted, there still remain immense deposits of it.

The price of this substance at the end of the last century was from 40 to 50 dollars the ton, and between 1820 and 1835 it was at times even less. About this period the monopoly of the Naxos emery was purchased from the Greek government by an English merchant, who so regulated the quantity given to commerce that the price gradually rose from 40 to 140 dollars the ton,—a price at which it was sold in 1846 and 1847. It was at this time that Mr Smith commenced examining and developing the emery formations of Asia Minor, until then unknown. And after making a report to the Turkish government, the monopoly of the emery of Turkey was sold to a mercantile house in Smyrna, and since then the price of this article has diminished to 50 and 70 dollars the ton, according to the quality. Such are the prices in the English market.

The different mines explored are those of *Naxos*, of an ancient date; of *Kulah*, commenced in 1847, and now abandoned for those nearer the sea; of Gumuch-dagh, commenced in 1847, and worked largely; and of Nicaria, commenced in 1850. From all these different places the emery goes to Smyrna, and from there principally to England, the vessels taking it at a very low price, as it serves for ballast.

The various mines belong to the Turkish and to the Greek government. The Greek government now sells its emery in lots of several tons. The Turkish government sells the entire monopoly of its mines, and consequently its operations are controlled by a single interest; but in all probability this monopoly will be done away with, in virtue of a commercial treaty existing between Turkey and the other powers. If this takes place, the price of emery will be still further diminished.

Of the different varieties of emery employed in the arts, that of Naxos is still preferred, and with reason, as it is more uniform in its quality than that coming from *Kulah* and *Gumuch ;* nevertheless, if the best qualities of that from the island of Nicaria are found in abundance, and that only sent into market, it will prove at least equal, if not superior to that of Naxos.—Vide *American Journal of Science and Arts*, vol. x., No. 30, 2d Series, p. 368, for further details.

On the General Vibration or Descent and Upheaval which seems, at a recent Geological period, to have occurred all over the Northern Hemisphere. By Dr Buist of Bombay. Communicated by the Author.

The whole of the desert betwixt Cairo and Suez bears the clearest evidence of having, at no distant period, been the bottom of the sea.

After a fall of rain an efflorescence of salt still appears on its surface. The gravel consists of rolled pebbles, mostly portions of the adjoining rocks. It is everywhere mixed with fragments of sea-shells. The desert, at the centre station, reaches an elevation of 800 feet, and shells are said to be found at the elevation of 2000, both on the African and Arabian side.* This most probably has been elevated at a remote period in comparison with the date of the upheavals along the shores of the Red and other seas about to be mentioned.

2. All around Suez there is a vast expanse of level plain, extending from two to twenty miles inward, diversified here and there with hillocks of drifted sand, obviously the effect of the wind. A section of the material of which the plain is composed is exhibited along the sea-shore. It is about eight feet above high-water mark, and consists entirely of sand, gravel, shells, perfectly fresh, and apparently of the same varieties as those on the beach. This upheaval extends, with little or no interruption, all the way to Aden, unless where the cliffs advance boldly on the sea. A similar beach, at a similar elevation, is formed all around the peninsula of Aden, and, though I have had no means of personally determining the fact, I have no doubt it will be found all along the Arabian

* Dr Wilson's Lands of the Bible.—Dr Hoffmeister's Travels.

coast, around the Persian Gulf, and so on to Scinde, and by the shores of Goozerat and Cutch. Of the delta of the Indies I shall have occasion to speak by and by, and so at present pass over Kurrachee. At Gogo, in the Gulf of Cambay, the raised beach is peculiarly conspicuous ; the gravel and shells are here cemented into a variety of stone, on which I have bestowed the term "Littoral Concrete," from its being always found near the shore, and from its resemblance to the artificial building material called concrete. At Gogo it overlays a huge mass of blue clay. With the interruption occasioned by the Delta of the Japtee, the raised beach, mostly consisting of the material just named, extends all along the shore to Bombay, and so on to the southward ; and, though I cannot speak from experience of the coast further south than 19°, I have great reason to believe it to be continuous, and feel almost certain that the specimens sent to me from Cochin, by General Cullen, belong to it. The upheaval, in all these cases, varies from six to nine or fifteen feet above high-water, rarely attaining the higher elevation. The same thing prevails around a large portion of the shores of Ceylon.

3. Darwin speaks of a calcareous deposit in New Holland, consisting of rock, which he thinks must have been formed by the drifting up of sand and shells over a mass of wood, the whole being afterwards consolidated by rain-water. This, I have no doubt, is an instance of the variety of formation, and a proof of the double movement under review ;* and it seems not improbable that the shell-formation of Madeira belongs to the same class of beaches, though of this I must not speak with confidence.†

4. The island of Mauritius is belted by an enormous coral-reef throughout its whole surface, excepting about ten miles. Between Savanne and the Bois-du-Cap the sea foams against a barrier of coral from five to fifteen feet in height, and wears it into the most fantastic shapes. At a considerable

* Journal of Researches by Charles Darwin.

† M'Aulay and Jameson's Journal, 1840. The Madeira wood is spoken of as being solidified ; if so, it must belong to a much more ancient date than that about to be described.

distance inland, almost concealed by the trees and shrubs, are two remarkable points or headlands of coral, from twenty to twenty five feet above the level of the sea. The observatory of Port-Louis is built upon a stratum of coral, ten feet above high-water mark. Blocks of coral too vast for being transported by any existing agency are found from 600 to 1300 feet inland, and which are cut off from the shore by elevated ridges.* The greater part of the numberless coral islands which lie scattered betwixt the Cape of Good Hope and Ceylon, the Chagos Archipelago, Seychelles, Laccadives, and Maldives, appear to have been elevated to their present level by the same upheaval by which the terraces now under consideration have been produced, of which I have no doubt abundance of traces will be found all along the shores of our eastern seas. Captain Newbold mentions the abundance of this class of phenomena on the coasts of the Mediterranean, where the shell-gravel, as in India, is being cemented into stone. Beaches hardening into stone prevail along the Straits of Messina.† The narrow isthmus connecting the Rock of Gibraltar with the mainland is obviously the result of an upheaval of the same age.

5. Amongst the numberless points where evidences of an upheaval are to be found in Scotland, are the following:—The railway betwixt Newhaven and Edinburgh cuts a large bed of shells about twenty-five feet above the level of the sea. A large bed of cockles, obviously *in situ*, is found at Borrowstounness,‡ in the Forth, at about 13 feet above high-water mark. Cockles live at from two to five feet below low-water. All around the shores of Fife to St Andrews, there are beautifully distinct exhibitions of upheaved beaches, several appearing in succession.§ These beaches, which have from St Andrews to Ferry-Port-on-Craig been covered with drift-sand, reappear along the banks of the Tay 10 or 15 feet below the surface of the carse-land, and west-ward by New-

* Transactions of the Geographical Society.—Jameson's Journal, 1841.

† Jameson's Journal, vol. xliv., p. 63.

‡ M'Laren.—Jameson's Journal, 1850.

§ Chambers' Old Sea-Margins. For the sake of brevity I have been compelled to speak very generally; it is with the lowest and most recent sea-margins which I am dealing.

burgh and Perth. Betwixt Errol and Invergowrie Bay, on the opposite shore, is a bed of cockles about three feet above high-water mark, corresponding closely in character, with that of Borrowstounness.* The Arbroath railway cuts and exposes the shell-bed from near Dundee to Broughty Ferry, after which it is concealed by the Sandy Downs. It reappears to the eastward of Arbroath, and again in Lunan Bay, and to the north and south of Montrose. Beyond this my researches along shore have not extended.

Two beaches are described by Mr A. Stevenson, off the Ross of Mull, near Skerryvore,† in the Firth of Clyde, and they prevail probably along much of the low part of the coast to the south.‡

6. The reasons why raised beaches are not at all continuous along our shores are very obvious. Where the shore is precipitous, and the water deeper at the bottom of the cliff than the whole amount of the upheaval, then, though the bottom of the sea might be raised by so much, and the water become to this extent shallower, there would be no actual beach, and the aspect of the coast would then nearly be the same as before, the cliffs having become just so much loftier. Beaches, originally existing, have been swept away when the whole of the material composing them consisted of sand, shells, or gravel, or when they rested on rock liable to decomposition, and the sea, in these cases, has once more approached its former cliffs or margin. Along the shores of Fife there are beautiful illustrations of beaches, well-preserved, where the rock was exposed in a way advantageous for resistance, and of their disappearance where it was otherwise. Near Crail the rock dips under the sea and exposes a surface well suited to withstand the surge; and there, accordingly, we have extensive raised beaches with the old sea-cliffs a considerable way inland. Near St Andrews, again, it is the reverse of this, the rock dips away from the sea, and the upheaved beach has been worn away, the waves now attacking and abrading the old sea-cliff. In this, again, ten or twenty feet up the cliff, we have caves, Lady Buchan's at St Andrews,

* Buist's Geological Survey of Perthshire.—Highland Society's Transactions, 1838.

† Jameson's Journal, 1840.

‡ Chambers' Old Sea-Margins.

and that of Kinketh to the south, which, doubtless, opened out upon the former beach, and were excavated, with those of Wemyss and Dysart on the north, by the surges of the ancient ocean.

7. The alluvium of the deltas of our great rivers can only be accounted for on the hypothesis of upheaval. I have rarely met with shell or gravel beaches off the mouths of our great rivers—the deltas or mud deposits, however, in these cases, take the place of the original beach, or covered or concealed it, or the whole has been eaten away again up to the verge of the purely fresh water deposit by the advance of the ocean. Streams which run sluggishly, or are partially stagnant, may give us sandbanks,—silt such as that of the Ganges, the Japtee, the Indus, the Nile, &c., is only precipitated when the water in which it is suspended is permitted, for *some time*, to remain in a state of absolute repose. Even were it otherwise, the deposit of silt must be restricted to the limits of the inundation, and yet in fact the inundation rarely extends over more than a mere fraction of the true alluvial delta. The same is the case with our carse-lands in Scotland, clearly consisting of river-silt, yet of silt which could only have been accumulated and consolidated under water in a state of repose. The level of our deltas and carse corresponds very closely with that of the most recent of our upheavals, of which, I have no doubt, they form a part.

8. I now come to the proofs of a descent having occurred anterior to the upheaval. It is, I think, nearly twenty years since Dr Fleming described the occurrence of beds of peat, with tree-roots, obviously *in situ*,* both in the estuary of the Tay and in the bay of Largo. The fangs and fibres of the roots are still entire, and fast in the ground as when alive; the stumps protrude some distance through the peat-bed. Dr Fleming seems at this time to have supposed that they were confined to the bed of the river: he does not appear to have been aware that the peat-bed was found everywhere under the clay of the low carse, surmounted by from 20 to 30 feet of alluvium. Peat-beds of a similar nature are found

* I quote from Dr Anderson's account of the Geology of Fife, given in Swan's Views of Fife, vol. i., p. 215.

covered over with a deep layer of alluvium in the valley of the Carse, and at Perth. Similar deposits occur at Mounts Bay in Cornwall, in Lincolnshire, and in Orkney. In 1837, in a report drawn up for the Highland Society, on the geology of the southern portion of Perthshire, I specially adverted to the circumstance of the occurrence of the beds of cockle-shells under the silt, and above the peat and tree roots which seemed to me only capable of being explained on the hypothesis that when the trees grew in the position now occupied by their roots, the surface of the land must have been, at least, ten feet higher than at present, so as to have placed them above the tide :—that a subsidence of, at least, twenty feet must have occurred, and that during this period the cockle-bed had come into existence; and, as the earth continued to descend, became buried in the mud which now covers it to the depth of ten feet. That the movement must have next changed its direction, raising the cockle-bed at least ten feet above its original position, bringing the Carse of Gowrie sixteen or twenty feet above the sea, and elevating the tree-roots to low-water mark.

9. The phenomena around us here, in Western India, exactly correspond with those of the Carse of Gowrie. The whole of our littoral formations consist of the concrete already alluded to, or of loose sand and shells. From three to ten feet under this—the depth varies—is a bed of blue clay, exactly similar to that with which our estuaries are being silted up. In a great majority of cases the blue clay is filled with the roots of the mangrove, a shrub which only grows within high-water mark, avoiding water of more than four or five feet deep. The fangs and fibres of the roots are perfectly entire, some of the thickest of them, indeed, are but imperfectly decomposed, most of them are converted into a substance like peat, and, when dried, break with a conchoidal fracture and semiresinous texture, something between jet and lignite. These roots and their arrangement is found to prevail all around the island of Bombay, on many parts of the shores of the island of Salsette, on the shores of the Gulf of Cambay, and at Kurrachee in Scinde. This state of things is not peculiar to creeks, bays, or estuaries, and can in no way

be accounted for by the ponding back of the water ; it prevails all around the shores of our islands and estuaries, penetrates into the interior as far as the gravel or concrete beds themselves, and is visible on those portions of our shores exposed to the full force of the ocean. It seems very probable that the New Holland trees described by Mr Darwin, and perhaps the Madeira wood mentioned by Dr M'Aulay, may belong to the same class as the roots I have described, though I have not felt warranted in adducing them as proofs of the hypothesis.

10. I am satisfied that, to this variety of objects, the lignite found near Cochin, in lat. 8° belongs, and that, were our shores examined, it would be found at intervals everywhere along them. In Scotland, at Perth, in the Carse of Gowrie,* in the carses of Falkirk and Stirling, and under the present city of Glasgow, and along the banks of the Clyde, boats and canoes have been dug out from under ten to twenty feet of alluvium, and still ten or twenty feet above the level of high-water. Mr Chambers concludes from these things, and, I think, most consistently, that the habitation of our island took place before the last thirty or forty feet of its elevation was gained from the ocean. May we not go further than even this? From the relations of these relics of human art to the peat-beds and submerged forests around, is it not possible that the *depression* under review was in progress within the human period?

11. The absence of roots, *in situ*, is no proof of a depression never having occurred ; at the present moment, for every fifty yards we have mangroves, we have at least 1000 where there are none, and on abrupt sandy or rocky shores, wherever, indeed, the locality is unfavourable for the collection of mud and the growth of vegetables, we can have no direct proof of depression.

12. If, as I have shewn, we have the old sea-margin of nearly uniform character, aspect, and elevation, presenting itself everywhere, it is not surely too great a stretch of inference to conclude that the depression was, like the upheaval, not local, but general, and that the two everywhere followed or accompanied each other.

* Chambers' Old Sea-Margins, p. 19.

13. This theory of double movement completely solves all the mysteries attendant on the formation of coral reefs,—the gradual descent permitted beds of coral of very great thickness to be formed, the ascent brought the whole again to the surface, or above it.

Synopsis of the Hunterian Lectures on Comparative Osteology. By Richard Owen, F.R.S., Hunterian Professor to the College.*

Lecture i.—*Introductory.*—The aims of Osteology illustrated by the comparative anatomy of the Temporal Bone.

Lecture ii.—*Development of the vertebral column.*—Notochord. Neural arch. Centrum. Arrested stages in Cartilaginous Fishes. Metamorphosis of vertebral column in Batrachia. Final purpose of the observed gradations of structure.

Lecture iii.—*Development of the Skull.*—Its stages exemplified, with special superadditions, in the Ammocete, Lamprey, Rays, Sharks, and Lepidosiren.

Lecture iv.—Persistent notochord in Lepidosiren and the earliest extinct Fishes. Forms of the centrum in ordinary osseous Fishes. Characters and divisions of the vertebræ of the trunk. Ribs and costal appendages. Modes of formation of the hæmal canal in the tail. Numbers of vertebræ in different fishes. Peculiar modifications of vertebræ at the two extremities of the trunk. No sternal ribs or sternum. Analogues of the sternum from the splanchno-skeleton and exo-skeleton. Combination of exo- with endo-skeleton in the vertical fins of Fishes. Modifications of the fin-ray characterising the *Acanthopterygian* and *Malacopterygian* orders of Cuvier. Modifications of the caudal fin characterising the *Homocercal* and *Heterocercal* fishes of Agassiz: prevalence of Heterocercals in the older secondary strata. Ichthyodorulites. Spine of Balistes. Adaptation of the unfettered vertically extended caudal vertebræ of fishes to their habits and place in Nature.

Lecture v.—Bones of the head in osseous fishes: large proportional size of skull, its general characters, and firm connection with trunk. Squamous sutures of most of the bones. Principal prominences and cavities of the skull. Classification of the bones of the head. Determination of the bones according to their special and

---

* We are fortunate in having an opportunity to lay before our readers the above important Synopsis, containing the present views of the highest authority on Comparative Osteology. The Lectures are now delivering in the Theatre of the Royal College of Surgeons of England by the illustrious Professor.

general homologies. Final purpose of the modifications of the cranial bones in Fishes.

Lecture vi.—*Skelctons of the Batrachia.*—Biconcave vertebræ of the perennibranchiate species: simplification of the skeleton as compared with that of Fishes. Cup-and-ball vertebræ of the caducibranchiates: reduction of the number of vertebræ in the anourous family. Determination of the bones of the head. Pectoral and pelvic extremities. Key to the nature of the pelvis afforded by that of the Menopome. Metamorphosis of the skeleton of the frog. General observations on the Batrachian skeleton. Its characteristics determine the nature of gigantic extinct species.

Lecture vii.—*Skeletons of the Ophidia*:—Characterised by the vast number and peculiar complexity of the vertebræ. Their processes and modifications in different species and different parts of the vertebral column. Ribs hollow, moveable, without sternal ribs or sternum. Long cervical hypapophyses, which form œsophageal teeth in the *Deirodon scaber.* Density and strength of the overlapping cranial bones: mobility of the maxillary, mandibular and palatine bones: modification of the upper jaw in poisonous serpents. Rudimentary scapular arch in Anguis. Vestiges of hinder limbs. Large fossil serpents. Compensations for absence of prehensile or ambulatory limbs.

Lecture viii.—*Skeletons of Lacertilia.*—Easy transition from Serpents to Lizards. Same procœlian type of vertebræ in both: their modifications in the cervical, dorsal, lumbar, sacral and caudal regions. Modifications of the atlas, illustrated by that in extinct Saurians. Structure of the skull in Lizards. Complex scapular arch. Locomotive extremities. Costal parachute of *Draco Volans.* True wings of extinct Pterodactyles.

Lecture ix.—*Skeletons of Crocodilia.*—Osteological distinctions between Crocodiles and Lizards. Characteristics of the vertebræ in different regions of the Crocodile. Determinations of the bones of the head: Complex eustachian canals. Scapular and pelvic arches and extremities. Illustrations of the extinct Reptilian orders of *Dinosauria* and *Enaliosauria.*

Lecture x.—*Skeletons of Chelonia.*—Solid texture of the bones in this order. Structure, development and homologies of the carapace and plastron. Peculiarities of the cervical vertebræ. Bones of the head. Edentulous beak-shaped jaws. Modifications of the skull in marine and terrestrial species. Scapular and pelvic arches and extremities. Antiquity of the chelonian modifications of the vertebrate skeleton, and their manifestation by extinct species of gigantic size.

Lecture xi.—*Skeleton of Birds*, essentially a modification of the Saurian type. Texture and general characters of the bones. Development and extent of the air-cells in the skeleton: ratio of pneumaticity of the bones to the powers of flight. Rapid and complete ossification. Extensive anchylosis of the bones. Characters of the vertebræ. Cervical, dorsal, sacral, caudal. Ribs and sternum. Fixation of the trunk. Length and flexibility of the neck related to want of prehensile power of the anterior extremities, to the functions of the beak and the position of the food. Analysis of the pelvis of birds : homologies of its constituent bones : utility of the separation and elasticity of the pubic bones. Relation of the anchylosed and expanded caudal vertebræ to flight. Determination of the bones of the scapular arch ; os humero-scapulare ; modifications of the bones of the wing, and especially those of the segment corresponding to the hand in relation to flight.

Bones of the leg : determination of the tarsus, metatarsus, and toes. Analogy between the metacarpus and metatarsus. Numerical relations of the toe-phalanges : their constancy exemplified in the didactyle foot of the ostrich.

Summary of physiological relations of the bones of the trunk and extremities in birds.

Lecture xii.—*Osteology of Birds continued.*—The skull; its special characters and relations to the habits and exigencies of the class. Smooth sutureless cranium ; its fossæ and foramina. Advantages of the single occipital condyle. Moveable articulation of cranium with face. Mechanism for such motion by the "ossa quadrata," "homoidea," and "communicatia" of Ornithotomists. Varieties in the size and form of the upper and lower mandibles : their relation to food and habits. Special homologies of the cranial bones of birds. Determination of the mastoids and prefrontals. General homologies of the cranial and other segments of the skeleton, and their relations to the archetype. Application of the osteology of recent birds to the restoration of extinct species. Large and unexpected accessions to Ornithology from these researches. Reconstruction of *Dinornis*, *Palapteryx*, and *Notornis*.

Antiquity of birds on the earth's surface shown by fossil footprints or "Ornithichnites."

Lecture xiii.—*General characters of the class Mammalia.*—Characters of the sub-classes *Implacentalia* and *Placentalia*, and of the implacental orders : Monotremata and Marsupialia. Principles according to which the Placentalia are divided into the sections *Mutica*, *Ungulata*, and *Unguiculata ;* and these into the orders Cetacea, Sirenia, Perissodactyla, Artiodactyla, Edentata, Rodentia, Insectivora, Cheiroptera, Carnivora, Quadrumana, and Bimana. Osteology of the Monotrematous genera *Ornithorhynchus* and *Echidna ;* Vertebræ of the trunk. Ossified sternal ribs. Persistent "vertebral" ribs in the neck. Analogy of lumbar and sacral Vertebræ to those of Reptiles. Marsupial bones. Skull. Analogy of smooth sutureless cranium with its ossified falx to that of birds. Essential mammalian characters of the skull : homologies of the cranial bones. Scapular arch. Bones of the fore- and hind-limbs.

Lecture xiv.—*Marsupialia.*—Geographical distribution and classification of this order. Its Osteology. Constancy in the number of the "true vertebræ." Extreme variety in that of the "false vertebræ." Peculiarities of atlas and dentata. Cervical spines of Opossum. Tail, modified to serve as a hand in the Opossums, as a foot in the Kangaroos, as a balancer in the Petaurists, and a rudder in the Thylacines. Composition and modifications of the skull: small cranium: long separation of the elements of the occipital and temporal bones. Vacuities in the osseous palate. Inflected angle of the jaw. Great diversity in the thickness of the cranial bones of different species. Scapular arch and bones of the fore-limb. Humerus generally perforated at the inner condyle, and sometimes between the condyles. Bones of fore-arm freely rotate in all. Scapholunar bone of Wombat and Kangaroo. Pelvis and marsupial bones. Bones of the hind-limb; their close correspondence with those of the fore-limb, in some species; degradation of the toes in others. Fossil Marsupials of Australia. Antiquity of the Marsupial type upon the earth.

LECTURE XIV. —*Cetacea.*—Their general characters compared with that of fishes; Vertebral column: anchylosis confined to the region of the neck. No sacrum. Peculiarities of ribs and sternum. The skull: singular development of the cranial and facial bones: their modifications in relation to locomotion in water and respiration of air. The hyoid arch. The bones of the fore-limb modified as a fin. Rudimental pelvic bones. Cranial and Vertebral characteristics of SIRENIA. Antiquity of the true Cetacea on the globe. The great Zeuglodon of Alabama. Fossil remains of Whales in the Suffolk crag: their unexpected value in agriculture.

LECTURES XV. and XVI.—*Ungulata, or hoofed Quadrupeds.*—General characters of this great natural group of Mammalia. Dental and osteological characters of the earliest forms introduced into this planet. Anoplotherium and Palæotherium, the types of the two natural primary divisions of Ungulata, viz., *Artiodactyla* and *Perissodactyla.*

Ruminant Artiodactyles.—Their osteology; peculiarities of cervical vertebræ. Structure of the skull: forms, structure, and growth of permanent and deciduous horns. Shedding and renewal of antlers. Modifications of the organs of locomotion. Recent introduction of the Ruminant type upon the earth.

Non-ruminant Artiodactyles, represented at the present day by the *Hog-tribe* and the *Hippopotamus.* Dorsal spines of the wild boar. Massive skeleton of the Hippopotamus; enormous facial part of the skull as compared with the cranium. Transition to *Non-ruminants* made by the Anoplotherium which retained many of the fœtal characters of the Ruminant.

Perissodactyla; three natural divisions of this group typified by the Horse, the Rhinoceros, and the Elephant. Leading peculiarities of their skeletons. Degradation of the feet from the pentadactyle to the monodactyle structure. Determination of the bones of the feet and of the retained digits. Former abundance and wide geographical range of the Proboscidian Pachyderms. The Elasmotherium, Dinotherium, Macrauchenia, and Nesodon.

Lecture xvii.—*Edentata or Bruta.*—Transition to this order from the Ungulata made by the extinct Megatheriod quadrupeds, which had both "hoofs" and "claws;" claws of great length in all the order: teeth, if present, without enamel; few other common characters. Leading divisions of the group typified by the Sloth, the Anteater, and the Armadillo. Their osteology compared: numerous dorsal vertebræ of the Unau; numerous sacral vertebræ of the Armadillo. Extreme flexibility and unusual number of vertebræ of the neck of the Sloth. Short and anchylosed cervicals of the Armadillo. Complex dorsal and lumbar vertebræ of Armadillos and Anteaters. Tail very long and prehensile in certain Anteaters: very short or wanting in the Sloths: of great size and strength, serving as an accessory hind-limb, in the Megatherioids. Its singular armour in the Glyptodons. Variable and inconstant bones of the skull illustrated by the conditions of the zygomatic arch in the Edentata. Long and edentulous jaws of the true Anteaters. Modifications of the fore and hind limbs for climbing, for digging, for uprooting, and pulling down trees. Dermal skeleton of the Manis and of the existing and extinct Armadillos.

Lectures xviii. and xix.—*Rodentia.*—Their numbers, extensive distribution, feeble and defenceless character and great fertility. Subdivided according to the rooted or rootless character of their molar teeth, which govern the nature of their food. Their osteology illustrated by the skeletons of the Squirrel, Beaver, Cavy, Jerbor, and Hare. Modification of the masseter muscle and of the mandibular articulation in relation to the gnawing powers of the order.

Insectivora.—Principal forms and osteological characters of this order illustrated by the skeletons of the Hedgehog, the Shrew, and the Mole. Modifications of trunk-vertebræ in the Hedgehog, of the cervical vertebræ in the Mole. Feeble or incomplete zygomatic arch; clavicles constant in the Insectivora. Relations of the sternum, clavicles, and massive fore-limbs, of the small and open pelvis, and feeble hind-limbs of the Mole to its subterraneous existence. High antiquity of the Insectivorous Mammalia.

Cheiroptera.—The characters of the types of the order show a modification of the *Insectivora* for pursuing their prey in the air. Vertebræ of the trunk. Skull: smooth thin cranium; moveable and inconstant premaxillaries; slender zygomata. Carinated sternum: scapula and large coracoid: powerful clavicles: ulnar patella: elongated fingers. Open pelvis. Bones of the hind-limbs. Peculiarities of the skeleton of Bats compared with that of other Mammals, of Birds and of Pterodactyles.

Lectures xx. and xxi.—*Carnivora.*—The families of this order typified respectively by the Dog, the Civet, the Hyæna, the Cat, the Stoat, the Bear, and the Seal. Characters of the cranium and jaws: Vertebral column: centres of motion indicated by spinous processes. Modifications of the extremities in the plantigrade Bear, the digitigrade Dog and Lion, and the pinnigrade Seal. Retractile claws of the Felines.

Quadrumana.—General characters and primary divisions of the order, and their geographical limitations. *Lemuridæ*, *Platyrhina*, *Catarhina.* Vertebræ of the trunk, of the tail, of the head. Progressive expansion of the cranium and diminution of the face as the series ascends, and the reverse as the individual grows. Retention of the immature proportions in the smaller Simiæ. Occipital and lambdoidal cristæ of the great Baboons. Change of position and plane of occipital foramen: expansion of facial angle. Complete and parallel orbits. Premaxillaries. Lower jaw. Hyoid bone: its singular expansion in the Howlers. Scapular arch and limbs. Pelvic arch and limbs: semi-inversion of tarsus. Carpal and tarsal bones compared. Antiquity of the Quadrumana. Concordance of geographical distribution of extinct with recent forms.

Lecture xxii.—Comparison of the Apes or Anthropoid Quadrumana with Man. Historical sketch of the knowledge acquired of the genera *Pithecus* and *Troglodytes*, and of their species. Skeletons of the adults compared. Skulls of the young contrasted with those of the adult. Deciduous and permanent dentitions. Sexual distinctions of the teeth. Cranial and dental characters of the great Chimpanzee (*Troglodytes Gorilla*). Its reported habits and ferocity. Its skull and that of the great Orang (*Pithecus Wurmbii*) compared with the skull of the Negro. Characteristics of the human vertebral column, pelvis, upper and lower extremities, and dentition, illustrated by comparison with those of the Orang and Chimpanzee. Osteological characters of the Æthiopian, Mongolian, and Caucasian races of Man. Question of the modifiability of characters considered in respect of the differences manifested in the skeletons of the Anthropoid Apes and Man. Recent introduction, and inadequacy of the transmutation-hypothesis to account for the origin of the human species.

Lectures xxiii. and xxiv.—Unity of plan of the Vertebrate skeleton demonstrated by a retrospective survey of the characters of the vertebræ in Fishes, Reptiles, Birds, and Mammals. The archetype-vertebra: its autogenous elements and exogenous processes. Extreme modifications of the type in the atlas and dentata: in the sacrum: in the carapace, and in the skull. Cranial vertebræ. Nature of limbs: their special, serial, and general Homologies. Conclusion.

On Recent Researches into the Natural History of the British Seas. By Professor EDWARD FORBES. Communicated by the Author.*

The Natural History of the British Seas has for a long time been a favourite subject of investigation. Within the last fifteen years, however, fresh inquiries have been set on foot, and the details of their zoology and botany worked out to an extent beyond that to which the examination of any other marine province has been carried. Numerous and beautifully illustrated monographs, treating of their fishes, cetacea, portions of the articulata, the mollusca, radiata, zoophytes, sponges, and algæ, have been published, either at private cost or by patriotic publishers, or by the Ray Society, such as the scientific literature of no other country can show. As these have all been the results of fresh and original research, they present a mass of valuable data sufficient to form a secure basis for important generalizations.

From these materials, and from the results of the inquiries into the distribution of creatures in the depths of our seas, conducted by a committee of the British Association, a clear notion may be formed of the elements of which our submarine population is composed. Extensive Tables exhibiting the sublittoral distribution of marine invertebrata, from the South of England along the Western coasts of Great Britain to Zetland, mainly constructed from the joint observations of Professor E. Forbes and Mr MacAndrew, are now preparing for publication as a first part of a general report from the committee referred to. The data embodied in these tables are the produce of researches conducted during the last eleven years, and registered systematically at the time of observation.

British Marine animals and plants are distributed in depth (or bathymetrically) in a series of zones or regions which belt our shores from high water mark down to the greatest depths explored. The uppermost of these is the tract between tidemarks; this is the LITTORAL ZONE. Whatever

* The above is an abstract of a lecture lately delivered in the Royal Institution of London.

be the extent of rise and fall of the tide, this zone, wherever the ground is hard or rocky, thus affording security for the growth of marine plants and animals, presents similar features and can be subdivided into a series of corresponding sub-regions; through all of which the common limpet (*Patella vulgata*) ranges, giving a character to the entire belt. Each of these sub-regions has its own characteristic animals and plants. Thus the highest is constantly characterised by the presence of the periwinkle, *Littorina rudis* (and on our Western shores, *Littorina neritoides*), along with the sea-weed, *Fucus canaliculatus*. The second sub-region is marked by the sea-weed *Lichina* and the common muscle (*Mytilus edulis*.) In common with the third sub-region it almost always presents rocks thickly encrusted with barnacles, so that where our shores are steep, a broad white band entirely composed of these shell-fish, may be seen when the tide is out, marking the middle space so conspicuously as to be visible from a great distance. In the third sub-region the commonest form of wrack or kelp (*Fucus articulatus*) prevails, and the large periwinkle (*Littorina littorea*) with *Purpura lapillus* are dominant and abundant. In the fourth and lowest sub-region the *Fucus* just mentioned gives way for another species, the *Fucus serratus*; and in like manner the shells are replaced by a fresh *Littorina* (*littoralis*) and peculiar *Trochi*.

Once below low-water mark the periwinkles become rare, or disappear, and the *Fuci* are replaced by the gigantic sea-weeds known popularly as tangles (species of *Laminaria*, *Alaria*, *&c.*) among which live myriads of peculiar forms of animals and lesser plants. The genus *Lacuna* among shell-fish is especially characteristic of this zone. In sandy places the *Zostera* or grass-wrack replaces the *Laminaria*. The LAMINARIAN ZONE extends to a depth of about fifteen fathoms, but in its lowest part the greater sea-weeds are comparatively few, and more usually the prevailing plant is the curious coral-like vegetable called Nullipore.

From 15 to 50 or more fathoms we find a zone prolific in peculiar forms of animal life, but from which conspicuous vegetables seem almost entirely banished. The majority of its inhabitants are predacious. Many of our larger fishes belong to this region, to which, on account of the plant-like zoophytes abounding in it, the name of CORALLINE ZONE has been applied. The majority of the rarer shell-fish of our seas have been procured from this region.

Below 50 fathoms is the REGION OF DEEP-SEA CORALS, so styled because hard and strong true corals of considerable dimensions are found in its depths. In the British seas it is to be looked for around the Zetlands and Hebrides, where many of our most curious animals, forms of zoophytes and Echinoderms, have been drawn up from the abyss of the ocean. Its deepest recesses have not as yet been examined. Into this region we find that not a few species extend their range from the higher zones. When they do so they often change their aspect, especially so far as colour is concerned, losing brightness of hue and becoming dull-coloured or even colourless. In the lower zones it is the association of species rather than the presence of peculiar forms which gives them a distinctive character. All recent researches, when scientifically conducted, have confirmed this classification of provinces of depth. When we have an apparent exception, as in the case of the submarine ravine off the Mull of Galloway, dredged by Captain Beechey and recorded by Mr Thompson, in which though it is 150 fathoms deep, the fauna is that of the coralline zone, we must seek for an explanation of the anomaly by enquiring into the geological history of the area in question. In this particular instance there is every reason to believe that the ravine mentioned is of a very late date compared with the epoch of diffusion of the British Fauna.

When we trace the horizontal distribution of creatures in the British seas, we find that though our area must be mainly or almost entirely referred to one of the great European marine provinces, that to which the Lecturer has given the name of CELTIC, yet there are subdivisions within itself marked out by the presence or absence of peculiar species. The marine fauna and flora of the Channel Isles present certain differences, not numerous but not the less important, from that of the south-western shores of England, which in its turn differs from that of the Irish sea, and it again from that of the Hebrides. The Cornish and Devon sea fauna and that of the Hebrides are marked by redundancies of species; that of the eastern coasts of England, on the contrary, by deficiencies. Along the whole of our western coasts, whether of Great Britain or Ireland, we find certain creatures prevailing, not present on our eastern shores. In the depths off the south coast of Ireland we find an assemblage of creatures which do not strictly belong to that province, but are identical with similar isolated assemblages on the west coast of Scotland. In the west of Ireland we find a district of shore distinguished from all other parts of our coast by the presence of a peculiar sea-urchin, to find the continuation of whose range we must cross the Atlantic to Spain. In such phenomena the Lecturer sees evidences of conformations of land, of outlines of coast and connections of land under different climatal conditions than at present prevail within our area, for an explanation of which we must go back into the history of the geological past. If we do so, we can discover reasons for these anomalies, but not otherwise.

The dredging researches about to be published go to shew that among our sublittoral animals the northern element prevails over the southern,—a fact indicated by the number of peculiar northern species; at the same time the southern forms appear to be diffusing themselves northwards more rapidly than the northern do southwards. This diffusion is mainly maintained along our western shores, and appears to be in action, not only in the British seas, but also along the shores of Norway. We must attribute it to the influence of warm currents flowing northwards, originating probably in extensions of the gulf-stream. The body of colder water in the depths of our seas preserves the original inhabitants of this area, remnants of the fauna of the glacial epoch, overlain and surrounded by a fauna of later migration, and adapted to a higher temperature. A curious fact respecting the marine creatures of the arctic seas of Europe, viz., that the littoral and laminarian forms are peculiarly arctic, whilst the deeper species are boreal or celtic, may be explained also by the influence of warm currents flowing northwards and diffusing the germs of species of more southern regions in the coralline and deep-sea-coral zones; for in the arctic seas the temperature of the water is higher at some depth than near the surface. On the other hand, we find in a region farther to the south than Britain, an outlier of the Celtic fauna preserved in the bays of Asturias, where it was discovered in 1849 by Mr MacAndrew; a very remarkable fact, and one appealed to by the Lecturer as confirmatory of his theory of an ancient coast extension between Ireland and Spain.

There is still much to be done in the investigation of the natural history of our seas, and many districts remain for more minute exploration. It is chiefly among articulate animals, and especially among worms, that fresh discoveries may be looked for. Yet even now new and remarkable forms of mollusca may occasionally be procured; and during the autumn of last year, in a cruise with Mr MacAndrew, no fewer than twenty additional mollusca and radiata were discovered in the Hebrides, and have just been described by the Lecturer in conjunction with Professor Goodsir. Among these is one of the largest, if not the largest, compound Ascidians ever discovered. In our southernmost province, fresh and valuable remarks have been conducted during the past year by Professor Acland and Dr Carus, who selecting the Scilly Isles as a field for exploration, have filled up a blank in our fauna.

(*To be continued in our next Number.*)

Optical Examination of several American Micas. By B. SILLIMAN, Junior, A.M., M.D., &c. Copy communicated by the Author.

Prior to the publication of the second edition of Dana's Mineralogy, little had been done in distinguishing the several species among American micas, and in allotting them to the various localities. In connection with Professor Dana, the writer, during the passage of the Mineralogy through the press, made a number of observations respecting the optical properties of such micas as were at that time accessible. A summary of these observations will be found in that volume.* Since that work was published, the writer has continued and multiplied his observations as far as opportunity has been found for prosecuting the investigation, while former examinations have been revised. The results of the whole research, as far as they are complete, are exhibited in the following tables.

Much yet remains to be done, not only in confirming and extending the present measurements and adding new ones from unexamined localities, but still more in reference to the chemical character of the several compounds, which from their great resemblance in leading physical properties have hitherto been generally confounded under a common designation. This branch of the inquiry is far the most laborious, requiring a large number of rigorous chemical analyses. A beginning in it, however, has been made by Mr Craw of the Yale Laboratory, who has completed three analyses of Phlogopites from New York. The results of his research, which are particularly interesting, will be found on a following page.

The physical questions connected with the micas embrace also the translucency of the several varieties in different directions, the effects of heat and magnetism in varying the angle of the optic axes, and the value of the latter under monochromatic light in all parts of the spectrum; and investigations on these points would well reward the observer.† I had proposed the subject last-mentioned to my friend Mr W. P. Blake, before my own observations were made, and he has recently planned and constructed for himself an instrument for observations and measurements of this sort. This instrument appears to me particularly well adapted for this

* Dana's Mineralogy, p. 690.

† A few experiments were made by the author, aided by Mr W. P. Blake, with Melloni's apparatus, to determine whether any relation in the transmission of heat existed between various micas corresponding at all to the different values of the optic axes. In these trials the mica plates were as nearly as possible of the same thickness, and they were placed so that the normal was parallel to the bundle of rays of heat. The instrument was so adjusted that the Locatelli lamp deflected the needle in 10″ of time 30° of the scale. Thus arranged, the following results were obtained:—

purpose, and with its aid we may hope for important advances in our knowledge of the physical relations of the micas.*

The instrument which I have used for the measurements given in this paper is a modification of the goniometer of Charles and Malus. It has a horizontal circle of about eight inches diameter reading to minutes, with a tangent screw and double readings. To the centre of the instrument has been adapted a simple contrivance for holding two tourmalines, and at the same time for securing the mica plate in the proper position. The tourmalines have both a horizontal and rotary movement, and are so arranged that the mica plate can be conveniently held between them in an unvarying position while the arm of the goniometer makes its revolution. The instrument is adjusted for use by bringing the specimen into such a position that the line connecting the optic axes shall be horizontal; and by turning the arm of the instrument through the requisite number of degrees, the two

Mica Examined.	Optic Angle.	Colour.	Needle deflected.	Per cent. of rays transmitted.
Muscovite of Grafton, ...	69° 30′	light brown,..................	19°–20°	57–60
Phlogopite, Pope's Mills,	7° 30′	white glassy, ...	12°-11°30	36–34·5
,, ,, ,,	15°	brownish yellow,...........	15°	45
,, Edwards, ...	13° 30′	yellow brown, ...	15°	45
Biotite (?) Topsham, Me., (*Probably Phlogopite.*)	....	deep reddish brown,......	13°–20°	39–36
Biotite, Monroe, N. Y.,...	....	dark green, almost black,	11°	33
Muscovite Royalston, Ms.,	57° 30′	dark brown, ...	21°	63
,, Paris, Me.,......	72° 30′	nearly colourless clear,...	21°-21°30′	63–64·5
,, Brunswick, Me.,	72° 30′	light brown,.................	21°-21°30′	63–64·5
,, Jones Falls, Md.,	67°	dark green,..................	18°-19°	54–57
,, Philadelphia,...	60°30′-61°	banded in hexag'l figures,	21° 36′-22′	64·5–66

When the crystal was placed so that the rays of heat passed parallel to the optic axis (thus the Grafton mica was placed at an angle of 34° 30′, the arrangement remaining otherwise as before), the needle was, on repeated trials, deflected 24°, equal to 72 per cent. of all the rays passing while in the other position (or with the normal parallel to the rays), only 60° passed.

From these few trials (which are regarded as only preliminary and approximative), it will be seen that an interesting relation apparently subsists of the sort looked for, and this last experiment is particularly worthy of confirmation by extending it to numerous varieties.

* Mr Blake presented his instrument and a series of measurements made with it to the Physical Section of the American Association at the New Haven meeting.

series of coloured rings of a common binaxial mica will come successively into view. A vertical cord placed in an open window is required to complete the arrangement; the instrument is so adjusted, that the cord accurately intersects the black dots of the inner coloured circle about one axis; a revolution is then made till the cord intersects in the same manner the other axis; the amount or angle of this revolution is the angle between the axes. With this arrangement there is no difficulty, after a little practice, in obtaining a series of measurements on the same specimen, varying from each other but a few minutes at most, without having recourse to lenses or other means of more accurately defining the field of observation or reducing the area of the coloured circles. Such modes of greater accuracy are important for the more delicate physical questions previously suggested; but for the purpose of mineralogical determination, the means just described are quite sufficient, since it is shewn that in a series of specimens from the same locality there is generally a difference of angle greater than any error of observation arising from the imperfection of the instrument employed.

Additional interest is given to this inquiry from a comparison of the chemical relations of the various species of mica and their corresponding differences in optical characters. For this reason we briefly recapitulate the divisions which are adopted by Professor Dana in the late edition of his System, and which are also given, with a recapitulation of the chemical formulas, on page 118 of vol. x. of Silliman's Journal. The species of mica now recognised are muscovite, margarodite, emerylite, euphyllite, margarite, lepidolite, phlogopite, and biotite. Of these, all but the last are binaxial. Our observations will be confined mainly to muscovite, lepidolite, phlogopite, and biotite.

1. *Muscovite.*—This name has been proposed by Dana to embrace those binaxial micas whose angle of polarization is between 55° and 75°, excepting however the lithia micas, which, having a peculiar composition and a very high angle, are included under the species lepidolite. The terms "oblique mica," "common mica," and "binaxial mica," formerly applied to this species, now fail to be distinctive, since we have

other oblique and binaxial micas which belong to different species. The optic axes in this species lay in the direction of the longer diagonal of the prism. It is much the most abundant variety, and is commonly found in granitic rocks.

2. *Lepidolite.*—This species embraces all the lithia micas, a group presenting, however, varied chemical characters which will probably be subdivided by future research. They are all binaxial, and as far as observed they yield a higher angle than any other of the species of this family, being 75°–76°. The blowpipe reaction for lithia, as well as its high polarization angle, enable this species to be very readily distinguished. Many of the varieties are easily recognised by their rose or peach-blossom colour.

3. *Phlogopite.*—This name was first proposed by Breithaupt for the yellowish-brown mica associated with serpentine, which is found at Natural Bridge, near Diana, in Jefferson county, New York. This species is distinguished by a polarization angle between 7° and 18°, the angle most commonly observed being 13°–16°; it rarely falls below 10°; in all cases the two axes are so near that both can be distinctly seen in the field at one view, and if examined in thin plates, and by a casual observer, it would be esteemed a uniaxial mica. The crystalline form is trimetric, and it occurs often in elongated and tapering hexagonal prisms, sometimes of enormous size, as in the well-known individuals from Pope's Mills,—specimens of which in the writer's possession are 5 by 8 inches in thickness, and perfect in form. The colour is usually yellowish-brown, bronze-yellow, and deep copper-red, sometimes greenish-yellow, and rarely white. Its cleavage resembles that of muscovite, but the laminæ are not generally so elastic. In chemical constitution it is a distinct compound, although but few analyses have yet been made of this species. Like the biotite, it is remarkable for the amount of protoxide bases which it contains, and the small quantity of alumina,—giving for the ratio of the oxygen its protoxides, alumina and silica, as deduced by Rose, $18 : 12 : 30 = 1 : \frac{2}{3} : 1\frac{2}{3}$ (more exactly 7 : 4 : 11, according to Craw), while in the muscovites it is generally 1 : 12 : 16. Its localities are much more numerous than was at first supposed; they abound

particularly in northern New York, in Canada, and in Morris and Sussex counties in New Jersey. One of the most noted localities of this species is Edwards, in St Lawrence county, N. Y., where it is found both colourless, of an eminent silvery lustre, and also of a rich brownish-yellow colour.

4. *Biotite.*—This species includes the uniaxial or hexagonal micas. Most of the varieties of this species are of a dark colour,—often black or greenish-black, and transparent only in very thin laminæ. Owing to this prevalent dark colour, it is often difficult or quite impossible to obtain satisfactory evidence of the optical character; and there is little doubt that some localities quoted in this article as furnishing uniaxial micas, should be in fact classed among the phlogopites. Only one American variety of this species has yet been analyzed, viz., that from Monroe, N. Y., by Von Kobell. They are generally magnesian micas, and have for the oxygen ratio of their protoxides, alumina and silica, the ratio $1:1:2 = \dot{R}^3 \dddot{Si} + \dddot{R} \dddot{Si}$. This species, and those anomalous specimens which are classed under it in the present article, but which probably belong elsewhere, offer interesting subjects for chemical examination.

Beside the *phlogopites* and *biotites*, properly so called, there are several micas which have fallen under my observation in this research, which are anomalous in character. These present under the influence of polarized light an elliptical coloured image, in which, however, it is not possible to bring out clearly the two poles of a binaxial mica, nor, on the other hand, the symmetrical cross of a uniaxial crystal. The divergence is too constant and too regular to allow the supposition that the ellipticity is due to a mal-position of the laminæ, or to a separation between the thin plates (remarked on as a cause of irregularity in certain crystals by Biot). The divergence of the axes in these exceptional cases is too decided not to attract the attention of the experienced observer, and still these specimens would probably, by most persons, be set down as uniaxial, especially in thin plates. Indeed, many phlogopites, when viewed in thin plates, appear so nearly uniaxial as scarcely to excite attention to their binaxial character, while in plates of suitable thickness they are easily measured. Those doubtful cases now under consideration are probably referable to an oblique crystalline form; but even here the study of a large number of specimens from the same locality is required before satisfactory inferences may be drawn. To this head I refer the deep reddish copper-coloured mica from Franklin Furnace, Sussex Co., New Jersey, which is found in white dolomite; also a mica of similar character from St Jerome, Canada; and that, well known to collectors, from Moriah, in Essex Co., N. Y., which is more remarkable than any I have seen for its deep smoky-red colour, as seen by transmitted light. But the most interesting specimens of this sort observed by me, are certain large crystals of a deep olive-green colour, from the Yale College cabinet, and which in our investigations have been referred to Monmouth, N. J., although their true locality is still doubtful. This mica is in very large rhombic crystals, oblique from an *obtuse* edge. P : M$=112°-115\frac{1}{2}°$, M : M$=122°-125°$, the angle of the basal edges is 119° 30′. Plane angle of P 119°. It has a cleavage parallel to the longer axis. The obliquity of the optic axes appears to be nearly as great as that seen in some phlogopites of equal thickness, but the dark colour of the mineral prevents a satisfactory examination. Should the character of this mica be confirmed by a set of good analyses, it must in all probability form a distinct species, as suggested by Dana.* This variety is not to be confounded with the well crystallised mica of Greenwood Furnace, which, as seen in ordinary specimens, is oblique from the acute edge (sections of distorted acute rhombohedrons), and which is regarded as a uniaxial mica.

Euphyllite, margarodite, and emerylite, have hitherto been found in quantities too inconsiderable, and in specimens generally too poorly crystallised, to furnish many measurements.†

(*To be concluded in our next Number.*)

---

* Mineralogy, p. 690.

† For the composition of these species, see Dana's Mineralogy.

On Interesting Changes of Internal Structure observed to take place in Solid Arsenious Acid, and other Solid Bodies.

The most remarkable property of arsenious acid, as Professor Hausmann observes, is this,* that as an amorphous body, without any admixture, and without losing its solid state, it experiences a change which makes it assume a totally different aspect. It has long been known, from experiment, that the transparent arsenic glass gradually becomes opaque, until it resembles porcelain. The substance, at first colourless, becomes white, the transparency disappears, and it becomes completely opaque; the beautiful vitreous lustre becomes feeble, and approaches the waxy. According to the experiments of Taylor and of Guibourt, the specific gravity at the same time diminishes. The former found that of the transparent acid 3·798, and of the opaque 3·529. The latter made the specific gravity of the transparent 3·7385, and the opaque 3·695. The hardness, also, is subject to change; the glass sometimes becoming pulverulent, so that its fracture is earthy, and the lustre quite gone.

Fuchs, in his beautiful work on Amorphism, has thrown out the conjecture that the glassy arsenious acid loses its transparency by virtue of a gradual change into a crystalline mass. Again, in his Natural History of the Mineral Kingdom, he asserts it distinctly; for he says "that the amorphous arsenious acid in time becomes white, opaque, and porcellaneous, and also becomes pulverulent, so that it can scarcely be recognised as crystalline. In order," says Professor Hausmann, "to ascertain whether the crystalline structure could be detected in the altered arsenic glass, I examined the crumbling outer crust under a magnifying power of 400, but could not perceive any trace of it. Though this experiment seems opposed to this view, yet I have been lately satisfied of its truth in the most convincing manner. In the year 1835, I received from the silver furnaces of St Andreasberg, through the kindness of

---

* *Vide* Journal of Geological Society, vol. vii. No. 25, p. 2, and Karsten and Dechen. Archiv. fur Mineral, &c., 1850, vol. xxiii. p. 766.

the director Herr Seidensticker (to whose management the arsenic works there owe their excellence), a specimen of the arsenic glass manufactured there, about two cubic inches in size, which he had broken off with his own hands immediately on opening the still warm apparatus, and had caused to be instantly packed up, that it might arrive as little injured as possible. The specimen, as I received it, had a distinct conchoidal fracture, without a trace of crystallization; it was transparent and colourless, and altogether of a glassy appearance. I laid it by in a drawer of my mineralogical collection, in a dry situation, close to my dwelling-room. A long time passed before I had leisure to lay my hands on it again; when I did so some years after, its appearance was surprisingly changed. Not only was the principal mass become porcellaneous, but also on the opposite side the parts next the surface had lost their clean conchoidal fracture, and to a depth of two lines had adopted a circular structure, so that the surface seemed rough and cracked. This change excited my surprise, which was greatly increased when, at the end of a few weeks, I found that not only had this acicular structure proceeded farther, and reached a depth of four French lines in some places, but also that the exposed side of the acicular masses was studded with a great number of distinct octohedral crystals (!); some of these crystals were half a French line in diameter. They were collected in small clusters, so as to give the whole surface a drusy, intumescent appearance. The acicular-shaped parts of the crust which were at right angles to the surface, passed insensibly into crystals, the groups of which seemed, as it were, to be pushed out beyond the surface. The crystals were white like the rest of the mass, but more lustrous and more translucent."

" Such a transformation of arsenic glass into a mass of well-formed crystals, is a most remarkable instance of molecular change in a rigid body, and is the more striking, since, apparently, it is not caused by any exterior circumstance, nor is attended by any change of constitution. It would seem that the molecules are put in motion by a tendency of the amorphous mass to pass from the condition of tension to that of repose and equilibrium, which is the characteristic condition of crystallization. This remarkable change also proves that nature can accomplish in time, what she cannot effect in a shorter space; a truth deserving to be remembered in all physical inquiries, and especially in geology."

"Some years after, I received from the silver mines of St Andreasberg a piece of arsenic glass recently formed, which I placed in my collection, near the specimen before described. It has also now acquired a porcellaneous appearance; but has preserved a perfectly smooth surface. I broke it across, in order to ascertain the condition of the interior. The inside is still perfectly glassy, the exterior only being changed. It is also to be observed, that the change from without inwards, has proceeded very differently in different parts. On one part of the surface, the thickness of the unchanged crust is scarcely appreciable; while, in other parts, the porcellaneous mass (in which the large conchoidal fracture is changed into uneven, small conchoidal) is two lines thick, with its interior limit ill defined. From this it seems to follow, that in very similar masses of arsenic glass there are certain differences of aggregation, which cause them to differ in their progress towards becoming opaque. On this it may depend, as well as on other determining causes, that generally the amount of change is independent of the length of time elapsed. For it is possible that arsenic glass might be kept a longer time than the piece I have described without exhibiting so remarkable a change."

Similar changes of structure are observed to take place in calcareous minerals, and *probably* changes of the same description occur in many other minerals, and also in rocks—among artificial substances exhibiting such changes we may particularise the confection called *Barley-sugar*. This substance, when newly prepared, has a pale wine-yellow colour and is more or less translucent. Its fracture is conchoidal, with a shining vitreo-resinous lustre. If kept for a time, its translucency diminishes, and gradually the conchoidal fracture disappears, and in its place the whole mass of the substance assumes a beautiful stellular radiated structure. Geologically considered, this topic is of great importance.—*Editor Phil. Jour.*

Remarks on the Scale adopted for the Ordnance Map of Scotland. By A. KEITH JOHNSTON, Esq., F.R.S.E., F.G.S., &c. Communicated by the Author.

The suitableness of a scale for any particular map, depends on the nature of the country to be represented, and the object for which the map is intended. A plan or map of a city requires, for sanitary and other purposes, to be drawn to a very large scale, and the Board of Ordnance has, very properly, fixed on five feet to a mile as most suitable for this object; but the map of a country, in order to be useful for general or scientific purposes, must be reduced to a much smaller scale, so that, in consulting it, the eye may take in at a glance a considerable extent of surface.

In 1840, Government directed that the Survey of Scotland should be published on a scale of six inches to a mile, in accordance with a desire expressed by several Public Bodies; for it appears to have been taken for granted that this scale having been adopted for Ireland, would also be found most suitable for Scotland, the fact of the difference in the nature and requirements of the two countries being quite overlooked.

On this subject we find it explained by Colonel Colby, that, since "Scotland does not require to be divided into areas like Ireland, it is proposed to give but little detail in the mountainous districts, leaving the map to be filled up as mining or other operations may require." * Hence the proposed map, in half of its extent, will contain only the information of one on a smaller scale, without any of its advantages. It is farther recommended, that "if ever a reduced map of Scotland should be done, it should be on a scale of three miles to an inch," implying that, although the large scale may have been required for Ireland, it is not at all necessary for Scotland. But so far is the map of Ireland on the six-inch scale from being satisfactory, that a reduction of it to the one-inch scale is declared in evidence to be "absolutely necessary." Lord Monteagle says, that "for certain purposes it is utterly useless, that many might not accept a gift of it if given to them, on account of its great size,—that no room could contain the map of even one county,—that if it could, it would not be possible to consult it,—that it is of little use for geological purposes,—and that it could not be so joined as to suit for laying down a line of road between two towns 20 or 30 miles apart." † The Map of Ireland referred to, is 150 feet long, and is comprised in 1907 sheets. Colonel Colby estimates that a Map of Ireland on the one-inch scale would be 23 feet high; and one on

* Parliamentary Papers, 1846, vol. xv., page 46. † Ibid., page 39.

the scale of one-third of an inch to a mile, would be eight feet high. The proportions for a Map of Scotland would not differ materially from these estimates.

A difference of opinion exists as to whether the system of contour lines could be advantageously introduced on a map on the scale of one inch to a mile. Mr Griffiths thinks that it could, and that its usefulness would thereby be greatly increased; but he considers that a scale of three inches to a mile would not contain the requisite details, and be quite unfit for this purpose. It appears from this evidence, therefore, that of the two maps proposed for Scotland, the one will be so large as to be of no practical use, and the other so small as to be quite imperfect. For as contour lines give none of the *effect* of mountains, the most elevated regions will be the same as a flat country; and since it is impossible to colour geologically the sheets on the six-inch scale, such a map must prove useless to the geologist and the scientific traveller.

The experience of Ireland further shews, that there is not the slightest probability of a map of the whole country on the one-inch scale ever being supplied by private enterprise, because it would not pay.

In corroboration of this, it is stated,* that "Greenwood, who published the counties of England on this scale, failed in consequence, and that the quarter sheet of the Survey comprising Manchester, one of the most extensively sold of any in England, cost the Government, for engraving alone, £313, that 2450 copies were printed, and that the return to the public will be just one-half of the original cost." It is stated, besides, that, even if such a publication were attempted, "it would be erroneous and inaccurate, if taken from the large printed sheets, on account of the contraction of the paper." If, therefore, a Map of Scotland on the desirable scale of one inch to a mile be not produced by the Government, its execution may be looked on as utterly hopeless. It may be proper to explain, that the scale of the *published map* does not in the least affect that of the original Survey, which must necessarily be large; and that the MS. maps may be consulted or copied, on payment of a fee, at the Ordnance Office.

Conceiving that it might be interesting to show the practice of other nations in this respect, I have drawn out a Comparative Table of the proportionate scales of the maps engraved from Surveys of different countries in Europe. From this it will be seen that the Map of France, in the preparation of which men of the highest scientific attainments have from the first been employed, is on the scale of three-fourths of an inch to a mile, or one-fourth less than the Ordnance Survey of England; and that all the others are smaller than this, with the exception of Bavaria, Würtemberg, and Baden.

* Parliamentary Papers, vol., xv., page 39.

But although the whole Map of France is comprised in 208 sheets, yet the necessity for a smaller map has been so urgently felt by the country, that the Government has been for some time engaged in preparing a reduction of the great map, to one-fourth the scale, or five miles to one inch.

This course has also been followed in Austrian Italy, a reduction of the original map to a scale of four and one-half miles to one inch having been recently published. The Survey of Switzerland, lately commenced, is published on a scale of only five-eighths of an inch to a mile.

Besides the considerations already advanced, the question of expense is materially affected by the scale. In the estimates for surveying and engraving the Ordnance Map of Lancashire,* the total cost on the one-inch scale is stated at £12,058, and on the six-inch scale £49,257, or more than four times the amount. The former is for a finished map with the hills shaded, and the latter for an outline with contours; the comparative cost of engraving alone, is, for the one-inch scale £2378, and for the six-inch scale £4515; from this it may be safely concluded, that the map on the one-inch scale would be less expensive by at least one-half, or the saving on Scotland would be about £400,000.

* Parliamentary Papers, 1849, vol. ix., page 1058.

Proportionate Scales of the Surveys of several Countries of Europe, conducted by their Respective Governments.

ORDNANCE SURVEY OF IRELAND.

6 inches to 1 mile $= \frac{1}{10,560}$ (Ireland, 2100 sheets.)

BAVARIA, WURTEMB., BADEN, HESSEN.

$1\frac{5}{16}$ inch to 1 mile $= \frac{1}{50,000}$ of nature.

SAXONY.

$1\frac{1}{8}$ inch to 1 mile $= \frac{1}{57,600}$.

ORDNANCE, ENGLAND.

1 inch to 1 mile $= \frac{1}{63,300}$.

FRANCE, BELGIUM, W. PRUSSIA.

$\frac{3}{4}$ inch to 1 mile $= \frac{1}{80,000}$ (France projected on the scale of $\left[\frac{1}{20,000}\right)$ (France 208 sheets, Belgium 22 sheets.)

AUSTR., ITALY, MODENA, PARMA, PIACENZA.

$\frac{3}{4}$ inch to 1 mile $= \frac{1}{86,400}$ (same scale as Cassini's France.)

HANOVER, E. PRUSSIA, SWITZERLAND.

$\frac{5}{8}$ inch to 1 mile $= \frac{1}{100,000}$ of nature.

TUSCANY.

$\frac{5}{16}$ inch to 1 mile, or 3 miles to 1 inch $= \frac{1}{200,000}$.

SARDINIA.

$\frac{1}{4}$ inch to 1 mile $= \frac{1}{250,000}$.

AUSTRIAN ITALY REDUCTION.

$\frac{3}{16}$ inch to 1 mile, or $4\frac{1}{2}$ miles to 1 inch $= \frac{1}{280,000}$.

FRANCE REDUCTION.

$\frac{3}{16}$ inch to a mile, or 5 miles to 1 inch $= \frac{1}{320,000}$.

GENERAL CHARTS, COASTS OF SCOTLAND.

Hydrographical Survey. } $\frac{3}{8}$ inch to 1 mile, or $2\frac{1}{2}$ miles to 1 inch. The Harbours and Bays vary from 3 in. to 12 in. per mile.

Inverness Firth, Kirkcud. Bay, Loch Eil 3 inches. River Tay $3\frac{1}{2}$ inches, Portree Harb. $7\frac{1}{2}$ inches, Loch Inver $8\frac{1}{2}$ inches, R. Clyde, 7 inches, Oban Bay 8 inches, Stornoway Harb. 9 inches, Aberdeen Harb. Ardrossan Harb. 12 inches.

The splendid Coast Survey of the United States is drawn to the scale of $6\frac{2}{3}$ inches to a mile, and the General Maps are published on a scale of $\frac{3}{4}$ of an inch to a mile, or $\frac{1}{80,000}$, same as that of France.

Meteorological and Astronomical Notices, March 1851. By Professor PIAZZI SMYTH. Communicated by the Author.

1. *Determination of the true Strength and Direction of the Wind.*—In Osler's anemometer, which is so very generally employed wherever a self-registering instrument is required, the strength and direction of the wind are recorded by means which mark on the paper the individual force of every little breath ; and as most strong gales consist of, as it were, a multitude of small gusts, blowing some this way and some that, the register of such a storm, in place of being represented by a well-defined curved line, by which the law of increase and decrease of the wind could be satisfactorily made out, is shewn rather by a broad band of oscillating pencil-marks, extending often over the greater part of the scale, in some minute terms of which it is required to ascertain the degree of the wind. In such a case, therefore, little precision can be expected in the deductions ; and, what is more, there is almost as much left to individual judgment in drawing a line through the mass of the pencil-markings, as if there had not been any instrumental method employed at all.

Now, there are few practical purposes for which we require to know exactly the nature of every little individual gust. If, for instance, a farmer wished to ascertain the degree of resistance which his haystack should be capable of opposing, so as not to be overturned in a violent gale ; then the minute results of the hard metallic surface of the pressure-plate of Osler's anemometer would not give him a true idea of the impact of the wind on the centre of gravity of the stack, when that wind has to strike, first of all, the loose feathery side of the straw, and then to be transmitted to the middle of the mass through such a large quantity of elastic material.

Again, if the effect of the wind on the great Exhibition building were sought, although the surface there is of the same nature as that of the anemometer plate, still the enormous extent of the building would have a powerful effect in equalizing the separate gusts ; and, as a whole, it would only be affected by the mean of them. In the case of a ship, too, we require not only the effect of the wind over a large surface, but during a considerable space of time, and therefore the mean, not the particular, strength : and in meteorological questions, it is of more importance to ascertain the general movement of translation of the air than any individual features of very small portions of it.

But while some instruments of the cumulative kind for giving the mean effects of the *strength* of the wind, as with Whewell's anemometer, have come into use, there was not only more difficulty in applying such a principle to the *direction* of the wind, but a fear was expressed that, if it were accomplished, we might lose the records of some of those important shiftings of the wind through an angle of 180°, which occur when the centre of a hurricane passes over the place of observation; and as this is a cardinal point in the hurricane theory, it was far too precious to be lost.

The Rev. H. Lloyd of Dublin has, however, removed the difficulties which had beset this case, by his investigations relative to a hurricane of the whirlwind description which passed over Ireland in November last. The Royal Irish Academy has recently established, it seems, a very extensive system of meteorological observation over the whole of Ireland, and the development of the nature of this "cyclone," is one of the first fruits which has followed. It seems to have passed almost centrally over the country, and the observations at all the stations are as coincident as in any case yet produced between the tropics; and have enabled Professor Lloyd to determine the whole diameter of the whirl = 500 miles, the preceding radius = 230 miles, the following radius = 270 miles, the velocity of the motion of the centre = 14 miles per hour, the velocity in the whirl = 40, the direction of the general progress from WSW. to ENE.,* and the diameter of the quiescent portion in the centre = 50 miles. These last numbers, therefore, combined with the rate of general motion of the whirl, shew that the change experienced in the direction of the wind, by being first in one half, and then in the other half of the whirl, cannot well be sudden, and that there is more than abundance of time for a very slowly acting cumulative machine, to adapt itself to the change of direction, without being caught by a current exactly 180° from a former one, and, therefore, perhaps acting for a time in a similar manner.

I do not know whether these facts shewed Mr Osler the possibility of applying the cumulative methods with advantage, but he has been engaged in applying such principles to his instrument for some time past. For the strength of the wind he employs Mr Edgeworth's elegant method of the hemispheres on the spokes of a horizontal wheel; all the concavities being turned in one direction, the centre of the hemisphere moves at one-third the velocity of the wind. In addition to this slowness of motion being in strong gales a material advantage over Mr Whewell's vane-wheel,—where there seems an impossibility in the little revolver to keep up on such occasions its full proportion for light airs,—there is the further merit of Edgeworth's being independent of the *direction* of the wind; and capable, when once a particular size has been agreed on, of being made by any mechanic in any part of the world, from numerical description only; and so exactly, that every such instrument can be used at once, without previous comparison with a standard.

For the mean *direction* of the wind, Mr Osler employs a plan

* This direction should carry the cyclone south of Edinburgh, and accordingly such appears to have been the case, by the record of the Osler anemometer on the Calton Hill; which also seems to shew, that it was gradually separating into two whirls; or that, at all events, two, partly mixed on their neighbouring sides, must have passed to the south of Edinburgh about that time.

similar to that by which the heads of windmills are turned round to the wind; viz., a small wheel at the back of the mill, and with its plane in the direction of the wind, so that in such a position it is not acted on at all; while if the wind changes its direction either way, it then begins to turn round the small wheel, which, acting through a train of wheel-work, is thereby enabled to turn the whole head slowly round, and place itself again in the sheltered position. This contrivance draws a very uniform line on the paper, in cases where the mere vane might be vibrating through angles of 50° and 60°.

The Rev. Dr Robinson, who pays almost as much attention to meteorology as to astronomy in the observatory of Armagh, has in use some arrangements of the anemometer, distinguished by the peculiar elegance and efficiency of everything which he takes up. For the strength of the wind, he uses Edgeworth's hemispheres, which he was the first indeed to bring forward, and he has been at much pains to ascertain the best size, and the exact value of the revolutions in terms of the velocity of the wind: as, by carrying it through still air at known rates; by immersing it in water moving at a measured speed, when the friction of the moving parts would be of far less moment than in the air; and again, by altering the central distances of two opposite hemispheres, to ascertain the proportions of radii that should give equality of *pressure* on the concave and convex surfaces, whence the consequent *velocity* may be easily deduced.

A practical difficulty, however, was found to rise in the rapid wearing of the lower pivot-hole of the vertical axis carrying the revolving-wheel of hemispheres; the pivot was made of steel, and worked in a conical hole in agate; but such was the wearing power of the ceaseless revolution of the spindle, day and night, that the agate was bored through in a short space of time. A second was made, but although well lubricated with oil, it soon shared a similar fate to its predecessor. A large piece of sapphire being then obtained by good fortune, was made into a bearing, but even that was actually bored through before very long.

The Doctor, therefore, then tried the plan of supporting the weight of the revolver on balls rolling inside a box, and this seems to answer perfectly, for while the friction is insensible, the wear or other alteration is also as small.

For the *direction* of the wind, Dr Robinson uses a simple vane, but connects the lower end of the spindle, where it enters the room, with a train of wheel-work, acting eventually on very large and light fanners, formed merely of thin lath fans, with paper pasted over them. The wheels are such, that for one revolution of the wind vane, the fanners might revolve say fifty times; and this velocity, combined with their large surface, causes them to offer an almost infinitely great resistance to any sudden impetus, as the impact of any little temporary gust. On the other hand, the small *weight* of the fanners offers so little resistance of inertia to any *constantly* acting force of even the smallest amount, that the *mean* effect of the lightest airs will be well registered.

Further, Dr Robinson has a plan of his own for overcoming the difficulty which most, if not all other, self-registering anemometers experience, from the wind sometimes going round the compass two or three times successively in the *same* direction ; in which case the pencil must be taken off any rectilineal scale on which it is expected to register. Mr Osler meets this difficulty, in some measure, by repeating his rectilineal scale three times over, so that if the pencil is registering a south wind in the middle scale, and turns round once either way, it will still be in a divided space, where it may go on registering a south wind ; if, however, the wind turns round, when no one is by, several times, this plan is manifestly incomplete ; and the tripling of the points of the compass has the bad effect of making the scale of each so very small, that all minute features are barely discernible. Dr Robinson, however, removes both objections by causing the wind,—not to move a pencil up and down in straight lines on a rectangular piece of paper, but—to turn a piece of circular paper round, while a pencil is being moved across it in a rectilinear direction by clock-work. The consequence is, that the wind may go round the compass as often as it pleases, either always the same way, or backwards and forwards — the pencil is still on the paper, and the registers are made on a very large scale. Small variations of the wind are exhibited, as well as changes of the mean, but not so as to interfere with the latter. And the interesting circumstance was thus ascertained recently by the Doctor, that the noise accompanying wind is mainly due to rapid variations in direction ; for one day lately, when there had been all the morning a violent howling storm, there occurred on a sudden so dead a silence, that the Doctor expected that the centre of a "cyclone" was passing over the observatory, and immediately ran up to the recording apparatus to see the expected change of the wind through 180° ; but he found that the wind was blowing just as strong as before, and in the same mean direction, but that the variations in that direction, which had been before exceedingly violent, had suddenly become hardly sensible.

The velocity of the wind is also marked on a circular paper, and while this plan has the advantage of allowing the sheet to be fastened on its metal disc with great ease, it appears to leave the wind much freer from friction than when it has to give rectilineal motion, instead of as here, a rotatory movement. As a specimen of the extreme sensibility of the machine, it may be mentioned that the revolving hemispheres are seldom or never at rest ; when not a leaf is stirring, when

> "There is not wind enough to twirl
> The one red leaf, the last of its clan,
> That dances as oft as dance it can,
> Hanging so light and hanging so high
> On the topmost twig that looks up at the sky,"

and only the smoke shews any symptoms of the atmosphere being otherwise than perfectly calm, the wheel is observed to have a slow revolution.

Altogether, therefore, Dr Robinson's anemometer may be considered to be the most perfect of any that have been brought forward, and its results are most precise and valuable, and for stationary observations on land, hardly anything more can well be desired. For travelling and nautical purposes, he has arranged a portable form of the Edgeworthian hemispheres, which is almost identical with one which I had contrived some years since for a naval friend, Capt. Cockburn, R.N.; and as a general notion seems to have prevailed that no anemometer but Lind's can be used at sea, and that the determinations must be far too rough to make the correction for the motion of the ship of any moment, it may be interesting to know that Capt. Cockburn, who has just returned from China, has had my Edgeworthian anemometer in constant use for three years, and that all the officers of the ship felt more and more confidence in its indications the more that it was used, and the more accurately that the correction for the motion of the vessel was applied, according to the law of the parallelogram of forces.

The chief difference between Dr Robinson's form and my own, is that in his, the recording wheels, instead of being permanently connected with the revolving spindle, are only brought into contact with it by the finger, during such time as the observation is being made; a method which would certainly be very conformable to the usual plans on which sailors observe the rate of the ship by the log line, and therefore likely to be received with more favour, and noted with more accuracy than any other.

2. *Hour of Observation for Mean Temperature.*—The importance of improving meteorological instruments for use at sea, can hardly be estimated too highly, for *there* alone, can we expect general laws to be found sensibly free from local irregularities; and every ship admits, with very little alteration, of being turned into a peculiarly efficient observatory for such phenomena. This has been recently insisted on by Mr S. M. Drach, who finds from his own theoretical researches, as well as Mr Glaisher's observations and deductions, that the important element of the mean temperature of the day can be satisfactorily and almost absolutely determined by four *equidistant* observations, whatever be the commencing hour. He prefers, however, 5 and 11 A.M., and 5 and 11 P.M., and in case of observations at sea, and in a uniformly moving ship, proposes to refer the mean of those observations to the ship's place at 2 P.M. He further points out that if the thermometer be likewise observed at 6 and 12 A.M., and 6 and 12 P.M., the horary increase may also be well determined.

3. *Luminous Meteors and Auroræ.*—As a transition subject between meteorology and astronomy, attention may be called to the circulars sent round by Professor Baden Powell, of Oxford, with

reference to "luminous meteors," and by the British Association, in the matter of the aurora borealis. Uniformity and sufficiency of observation are extremely necessary to be attended to by all who undertake these subjects, and they only have to write, it seems, to Prof. Baden Powell, at Oxford, or to Prof. Phillips, at York, for copies of the forms. The following is an enumeration of the subjects to be attended to in regard to tables :—

*Observations of Luminous Meteors.**

1, No.; 2, date; 3, hour; 4, place; 5, appearance of magnitude; 6, brightness and colour; 7, train or sparks; 8, velocity or duration; 9, direction or altitude; 10, general remarks; 11, observer; 12, reference.

Simultaneous Observations of Aurora Borealis.†

Place____ Lat. N.____ Long. W.____ Elevation above the sea in feet______

1, Day, hour, and minute, by Greenwich mean time; 2, definition of the object specially observed, as arch, beam, corona, or point of convergence of streamers; 3, position of the object by stars; 4, position of the object in altitude and azimuth, by instruments;

* *Directions.*—No. 3. Under the head "Hour," state Greenwich mean time if known; if not, what time is meant. No. 4. Give the latitude and longitude of the place of observation. No. 5. Compare with magnitude of stars; or with the apparent brightness or diameter of planets, the moon, &c. No. 6. State whether the brightness increased or decreased during the appearance. No. 7. Distinguish the kind of train; whether continuous or broken; whether a real train, or only an optical impression, &c. No. 8. If possible, give the difference of hour, minute, and second appearance and disappearance. No. 9. The direction as compared with any known fixed stars; or the altitudes, or if possible, the right ascension and declination of the points of appearance and disappearance, or at least the points of the compass.

† Notes.—1. Auroral phenomena have been found most frequent between the hours of 7 and 12 P.M.; the greatest number at 9, or soon after. It is recommended that at every 10th, 20th, 30th, &c. minute of Greenwich mean time (as 8·0, 8·10, 8·20, &c.), the definition and position of arch, beam, or point of convergence (as being most suited to measurement and likely to be recognised at distant places), be recorded in columns 2 and 3, or 2 and 4; and that at the 5th, 15th, 25th, &c. minute, the state of the magnetic needle be recorded in column 5; the other minutes being left to be employed by the observer, on luminous clouds, falling stars, and any other phenomena which he may deem interesting, for the notice of which columns 6, 7, and 8 are intended.

2. If an arch be observed, state if it appear like a simple luminous ring across the sky, or a ring divided transversely into cross ribs, or a mass of light surmounting a dark circular segment. These phenomena are not to be confounded. Luminous clouds, clouds illuminated at intervals, streamers, or systems of streamers, may be also observed.

3. In marking the place of the arch or beam or corona among the stars, a globe or planisphere, set to the day and hour, will be found very convenient. *Three well-determined distant points* in the course of an arch or beam (for instance, the point of greatest altitude and its two extremities), will give its position. The breadth of the arch or beam may be estimated by the angular distance of neighbouring stars. If an arch or beam drifts, advantage may be taken of observing when the arch appears exactly *between* two known stars. The *motion* of arches and beams is often very apparent and capable of actual measure. Arches often appear to move southwards; more rarely turn on an axis which approaches to the vertical.

5, effect on magnetic needle ; 6, appearance as to brightness, colour, and texture ; 7, falling stars ; 8, other remarks.

4. *The Asteroids.*—With reference to the now so numerous family of asteroids, the Rev. Temple Chevallier, writing lately from Durham, says,—

"Our observer, Mr Carrington, has lately computed with much care the positions of the orbits of all the small planets, with relation to two planes at right angles to the ecliptic and to one another, and has constructed a model representing them all. This has brought to light a remarkable relation, hitherto, I believe, unobserved.

"All the orbits are so arranged, both in reference to their planes and the position of their points of perihelion, that all the orbits approach very nearly to one another in about heliocentric longitude 185°.

"This was made out for the first twelve planets before the discovery of Egeria; and I was very anxious to see whether her orbit would conform to the rule. It does so, in a very remarkable manner, the path of Egeria coming abruptly down to the point of concourse as that of Pallas rises still more abruptly from the same region of space.

"Whether this relation may exist or not for planets still to be discovered, or should fail, it must have a marked influence on the perturbations of those planets, and may lead to remarkable consequences in the theory of their physical connection."

5. *Bond's Third Ring of Saturn.*—In the matter of Bond's new ring of Saturn, it seems that the important character of the discovery was much underrated in the last notice, for although no accurate series of measures has yet been published, it seems certain

---

4. For observations of this kind, the instrument proposed by Prof. Challis (Brit. Assoc. Reports for 1848) may be used. Persons accustomed to geometrical considerations may arrange methods suited to the place of observation. A pocket compass, well used, will give azimuths *nearly*. A vertical pole will enable an adroit person to get altitudes and directions accurately by adjusting and marking his own position.

5. The horizontal variation of a needle (suspended by silk fibres, or a single hair, or *very delicately* pivoted) is often very sensible in aurora, and deserves careful attention. The needle must be absolutely free from shake by currents of air, and not be subject to *variable* influence of iron. It is desirable to observe also the dipping-needle, but for that purpose adequate apparatus is only in few hands.

6. Here describe the appearances and variations of the arch, beam, or corona, which is the subject of observation. Are stars seen through the aurora? Is the light steady, or subject to flashings, or broken by dark patches, or marked by pulsations? Is there redness? If there be various colours, how are they disposed? How do they succeed one another?

7. If falling stars are noticed, state the precise hour, minute, and second, of the occurrence, and give their appearance and path among the stars.

8. Notices and suggestions not provided for in previous columns may be placed here. The indications of the barometer and thermometer, and of the wet and dry-bulb thermometer; the state of the weather *previously*; the direction of the wind; the probable height of clouds, &c., are worthy of record.

that there is decidedly a new ring, in a place where there was assuredly none ever seen before, namely between the inside of the inner of the old rings and the body of the planet; the new ring, too, is described as being almost $\frac{4}{10}$ of the breadth of that space, and apparently thicker than the older ones, but very much fainter in point of illumination; the feebleness of its light, indeed, is what makes it so difficult an object still for any but a very powerful telescope and a practised eye, under favourable atmospherical circumstances.

Yet it has been seen by telescopes much inferior to those which have been employed on Saturn before, and has appeared in a place which has been the subject of such frequent inspection and observation, that it becomes a matter passing strange that it should never have been seen before. It was first detected by Professor Bond in November last, but was almost simultaneously perceived in this country, first by Mr Dawes, and then by Mr Lassel; though these gentlemen appear to have been several days later than the American, and not so decided as to the nature of the novel appearance which they witnessed: still it is an interesting trait in the history of astronomical discovery, that again and again the American and British observers are running each other so very close. Mr Dawes remarks, that as the side of Saturn's ring which is now turning up to our view has been in the shade of the sun for the last fifteen years, some mist or fog may thereby have been generated which may account for the appearance just now of the new faint ring; but that although this might somewhat relieve us of the difficulty of this ring not having been seen by the same observers with the same telescopes when the other side of the ring was turned up a few years since, it does not explain satisfactorily how it came about that Sir W. Herschel, who had the opportunity of watching Saturn under precisely similar circumstances to the present, and used it to good purpose, too, with his forty feet reflector, did not suspect the existence of anything of the sort.

The question would probably be completely set at rest if the ring could be observed transiting before a star, as its opaqueness or transparency would then become immediately evident. But this is an event which we may wait many years for in this climate and with small telescopes; while if we had a large reflector on a high tableland in the tropics, the number of visible stars would be so greatly increased, that an opportunity of determining this interesting question might soon be obtained.

Meanwhile we may be well content with the frequently recurring proofs of the excellence of the modern telescopes, reflectors and refractors; for the last disappearance of Saturn's ring produced the discovery of an eighth satellite, and its reappearance has been attended by another discovery, and of a most startling and unexpected description, as we seem almost driven upon the necessity of some formative process at work even still.

6. *Great Reflector for a Southern Country.*—With reference to the allusion made in the last Notices to the refusal of the Government to entertain the recommendation of the British Association to send a large Rosseian telescope to a southern country, a friend, upon whose opinion I place very great weight, has informed me that he considers that I was decidedly wrong in attributing the refusal of the Govern ment to their dislike of the *expense;* he says that it was not their *expressed* reason, nor does he believe it to have been their influencing reason; but that rather he should fancy, though he does not profess to know what the private opinion of Government might be, that they considered the scheme was not yet in a practicable condition; that reflecting telescopes had not yet been brought to the proper certainty of performance under ordinary official hands, nor had any sufficient and appropriate person yet appeared for being appointed to the office.

I gladly take, therefore, the first opportunity of expressing my regret for having erroneously supposed the existence of such a motive as that of *expense,* without sufficient examination of reports; and of giving a full statement of what the influencing reasons of Government may reasonably and favourably be considered to have been in this matter. Whether those reasons be right or wrong, the *real* cause which has led to the refusal of a request so fully discussed and highly approved of by such a body as the physical section of the British Association, and by the Royal Society, cannot be too extensively known; if right, it may lead to the attention of astronomers being turned to the weak points of the proposition for its improvement; if wrong, it may lead to the idea being brought up again and more successfully urged.

If we are to judge of the proper time for such a request having arrived, by enquiring whether the reflecting telescope has reached perfection, or even the highest degree of it eventually attainable by man, then the step of the British Association is decidedly immature; but on such principles the time will never come, for to suppose that it will arrive certainly within any definite period is as rash as the conclusion which the French astronomers came to in the days of Louis XIV., when, concluding and boasting that astronomical instruments had then reached their ultimate degree of excellence, they proclaimed that the time had come for achieving the conquest of southern skies, and the Abbe de la Caille was accordingly sent to the Cape of Good Hope for that purpose. But although he used in a most masterly style all the instruments he was furnished with, his voyage did not render one whit less necessary the recent expedition of Sir J. Herschel, with a telescope compared to which, La Caille's was not to be mentioned. The history of astronomy, shewing as it does,—that every successive proclamation of instruments having obtained their utmost available excellence, has ended in the confusion of all advocating such views,—demonstrates that the real or supposed shortcomings of the modern reflector from ideal perfection are not the grounds for judging of the propriety of such a proposition as that of the great "Southern Reflector." We should rather inquire if any very decidedly superior instrument can be made now, to the last employed for a similar purpose.

Then the decisive answer can be given, that instruments larger, and cheaper, and better can be made now than at any former period in the history of the reflecting telescope; and when every person who has had a hand in the making of the modern reflecting telescope, as Lord Rosse, Dr Robinson, Messrs Lassel, Nasmyth, and Grubb, is decidedly of this opinion,—it seems strange that the Government, which can hardly be expected to be very conversant with the minute details of the practical accomplishment of such an affair as this, should inform them that they are mistaken in their idea of the practicability of what they have been engaged in for so many years; at least a committee might have been formed to communicate with these gentlemen, if the Government was really favourably inclined, and to ascertain how the fancied difficulties could be possibly met and overcome.

That *larger* reflectors may be made now than ever were before, one has only to look to Lord Rosse's; that they can be made far *cheaper* I had abundant evidence recently, on being shewn one of 9 inches aperture, sold to George III. for £760, while another of 20 inches aperture, made the other day by Mr Nasmyth, and which, so far as the increase of size goes, should have cost at least five times as much, viz., in the proportion of the squares of the apertures, not to say anything of superior excellence of quality, cost him only £60 in work and material, exclusive of the value of his own time in superintending.

That the telescopes are also decidedly *better*, one has only to look at the superior quality of the modern metal, so white and lustrous, and free from pores, while the figures given to the surface by machine in place of hand polishing have had their surpassing quality undoubtedly tested by the performances of the instruments,—by the resolution, for instance, of the nebula in Orion, although far from favourably situated in these latitudes, and by the complete separation of the second and third stars of γ Andromeda, as well as by the appearance of small stars very close to large ones.

The mountings of such telescopes have also improved, as witness the simplification and solidity introduced into altitude and azimuth stands by Lord Rosse and Mr Nasmyth, and the clock-moved equatorial mountings under revolving domes, of Dr Robinson, and Messrs Lassel and Grubb.

Doubtless there may be much room for improvement still, and the variations and contrasts in the above mountings would seem to indicate that the problem of devising the best form has not yet been completely solved; and I have no hesitation in confessing that not one of the above stands is exactly conformable to my ideas of what it should be, or rather what I should like to employ, if I had to make the observations; but nevertheless I should not attach any weight to such an opinion, if any one else had to use the telescope; for according to each person's peculiar physical conformation and peculiarities, he can do the best work with some different form of instrument to his neighbour. And here is a generic difference between meridian and extra-meridian astronomy; in the former, almost everything depends on the absolute perfection of the mechanical action of the instrument, and hardly anything on the skill of the observer; while in the latter, the case is so completely reversed, that a life spent in a meridian observatory seems almost to unfit a person for undertaking extra-meridian observations. A person accustomed for years to nothing but mural circles and transit instruments would be utterly aghast at being called on to make any accurate observations with one of Sir W. Herschel's, or even Sir J. Herschel's reflectors, they would be to him a bow of Ulysses, which, to whatever good purpose it could be bent by its owner, would be powerless in the hands of any one else. The results produced, should form the only test of the sufficiency of the instrument of any particular observer, for that which suits the idiosyncrasy of one person may be distasteful and even impossible to another.

"Never use flat brushes," said Haydon in one of his Edinburgh lectures on painting; "no one can produce fine pictures with flat brushes: Sir Thomas Lawrence used flat ones, you should employ round ones." But who, on comparing the works of Sir T. Lawrence and Mr Haydon, would consider the precept borne out by the examples? Again, who could have painted decently with Etty's brushes, with the hair straggling out in all manner of ways? and yet none of his cotemporaries could approach him in results produced by the brush. But what should we think, if he had been prevented from undertaking any large work for a public body, until he should have satisfied perhaps a board of his brother artists, or perhaps a board who had never painted at all, that his brushes were of the fit and proper description wherewithal to produce a good painting?

Moreover, with regard to the objection raised that the coming man has not yet appeared, that no one can be pointed out either with all the qualifications for making the telescope and using it afterwards, or, if possessing them, that he is not at the command of the Government,—the proposers of the measure were prepared, if desired, to make the telescope themselves, and to furnish it complete to Government, so that there need have been no difficulty on that score; perhaps even to have recommended the observer; but it is not the usual practice of scientific societies to dictate when asking a favour. The minister was informed most distinctly what was the desideratum of the Association; was certified of its practicability by one who has borne a large share in making and using the largest modern reflectors; and even had an estimate given to him. Enough was done, therefore, if he had been disposed to comply; and it would have been the usual course to request the British Association or the Royal Society (the other petitioning body) to name a committee to confer with him. But this course was not pursued, and the answer which was given, has been a heavy blow and a serious discouragement to the prosecutors of sidereal and telescopic astronomy.

Nor are matters much better when we turn to the East India Company; for it seems that though Captain Jacob has received permission to remove a part of the establishment of the Madras Observatory to the Nilgherry hills, to work a large reflecting telescope there; still he is distinctly told that he must not expect that the Honourable Company will be at any expense in providing the said telescope;—it must be made by himself, and at his own expense. Now, even allowing that, with his skill at the foundry and at such lathes as he could procure the use of in India,—he might turn out an instrument of sufficiently good quality to meet the requirements of present times, still how small must necessarily be its size and consequent power, when limited only by the possible extent of savings from his salary.

7. *Observations of α Centauri and α Piscium.*—In another department of the observatory, the Court of Directors seems to have been much more liberal, and has provided it with an equatorial instrument well adapted for accurate observation of the larger double stars; and from a recent letter I extract my friend's observations of α^1 and ^{2} Centauri for 1850, not only on account of the rapid motion of the stars during the interval, and the great interest which now attaches to them, but on account of the extreme goodness of the observations themselves, equalling, if not surpassing, as they do, anything that I have before met with in double star observation; and though the actual discovery of new stars and satellites may be a matter of more popular interest at the time, and even induce nations to contend for the honour of priority, yet the accuracy of numerical measures of known bodies is a matter deserving of equal praise, and will in future astronomical ages be probably considered of greater consequence, or at least will be more thankfully received, and more preciously treasured than the mere date of the first discovery.

"Below," says Captain Jacob, "are all my last year's observations on α Centauri, collected in groups of three or four sets each. They form rather a pretty series, unbroken in position, and with only one break in the distance, though three of the intervals are but of three weeks each. If we get another as good for this year, we ought to know something about the orbit.

$\alpha\ ^1$ and ^{2} CENTAURI.

Date.	Position.		Distance.
	°	′	″
1850·292	247	14	6·53
·449	—	48	·42
·603	248	50	·26
·662	249	19	·02
·887	250	5	5·86
·944	—	25	·82
·997	—	56	·88

"Will you favour me," he further says, "with your opinion regarding α Piscium? Annexed are all the observations accessible to me, and they present strange anomalies.

α PISCIUM.

Observers.	Date.	Position.		Distance.
		°	′	″
Sir William Herschel,	1779·80	337	23	5·12
— —	1802	333	0	
Struve, ...	1819·90	340	48	
Sir John Herschel, Sir J. South,	1821·93	335	33	5·43
Captain W. H. Smyth, .	1834·92	334	42	3·6
— — . .	1838·87	333	24	3·8
W. S. Jacob, . . .	1842·94		00	4·04
— — . . .	1845·84	330	12	4·20
— — . . .	1850·96	329	26	3·63

"But is it possible to pronounce the pair as constant? I think not: that would involve an error of 5° in my last sets, and of 6° in Struve's observations. On the other hand, if we take the progression indicated by the observations from 1819 onward, and carry it back to Sir W. Herschel's time, we must then suppose that he made an error of 8° or 10°, which seems inadmissible. Is it then conceivable that the pair have moved nearly 180° between 1802 and 1819? The ellipse being so highly elongated that this portion of the orbit covers no greater area than the 4° or 5° described between 1780 and 1802, and a less area than that of the 10° between 1820 and 1850. The stars are near enough to equality, I think, to admit of the hypothesis; but we shall know more about it in a few years' time."

This passage was certainly only sent for a private opinion, not for publication; but as it seems a perfectly legitimate conclusion, and one which, if it should turn out correct, strongly exemplifies the propriety of Sir John Herschel's recommendation that double stars should be *constantly* watched and measured, that even with the slower movers, an observation every four years is not enough,—the sooner it is printed, and the more extensively it is known and attended to, the better for the cause of the science.

8. *Royal Observatory, Cape of Good Hope.*—Some excellent observations of α Centauri and other double stars of the southern hemisphere, have also been sent home by Mr Maclear, Astronomer-Royal at the Cape of Good Hope; he expects shortly to forward the results of several years' observations undertaken on stars in that neighbourhood, with the expectation of their shewing a sensible parallax. He is likewise engaged with the meridian instruments in an extensive and searching investigation into the real places of all those southern stars in the British Association Catalogue, which depend on an in-

sufficient number of old and generally inaccurate observations. The result shews the necessity for such a revision, and in addition to errors of places in right ascension and declination, there are as many in the estimation of the magnitudes of the stars.

The difference between the magnitudes given by La Caille, and those observed by himself lately, appears to have given Captain Gilliss, U.S.N., (now conducting the American astronomical expedition to Chili), the idea, that there must be greater number of variable stars in the southern hemisphere than in the northern ; but when we consider the haste with which La Caille's observations were made, the imperfection of his instruments, the number of errors detected by Mr Maclear in his places, and the frequent entry as a star of what is undoubtedly a nebula, or a cluster of stars, and sometimes a very loose and straggling cluster,—we cannot allow that any proof has been made out of such a physical difference between the stars of the south and the north.

And with regard, too, to the supposed difference between the shape of the world in the two hemispheres, which appeared from La Caille's measure of an arc of the meridian at the Cape,—Mr Maclear's repetition and extension of that operation has, in an approximate calculation, completely negatived that supposition ; and the more exact computation of the great work is meanwhile being rapidly pushed on, and will be received on its completion as a most precious addition to our knowledge of the real figure of the earth.

SCIENTIFIC INTELLIGENCE.

GEOLOGY AND MINERALOGY.

1. *Reptilian Footprints in the Lowest Silurians.*—Mr Logan who has conducted so well the geological survey of Canada, at present in London, a few days ago unpacked a case containing a series of slabs and casts exhibiting impressions of footsteps and trails in the Potsdam Sandstone, the lowest fossiliferous bed of the Lower Silurian or transition class of rocks hitherto reached in America. We are informed these very interesting specimens have been seen and examined by one of the most distinguished palæontologists of this country, who considers them as *Reptilian ;* in all probability of an animal allied to *Emys.* Thus we have indications of air-breathing vertebrates at the first dawn of animal life.

2. *Map of Iceland.*—The hydrographical surveys of the entrance of the Cattegat, and of the Great Belt, have been completed ; but the recent war with Prussia has delayed the contemplated examination of the west coast of Iceland, and other important works. However, the large map of this interesting island has been completed, and a copy has been promptly forwarded to

us from Copenhagen. It is on four sheets, upon a scale of $\frac{1}{480,000}$, and is highly creditable to the late Colonel Olsen, the superintendent; Professor Gunnlaugson, the draughtsman; and Messrs Scheel and Frisak, who conducted the triangulation. This is, without doubt, the most important contribution to the exposition of the natural condition of Iceland that has yet appeared; and therefore it is that I may dwell a moment longer on it than I would upon works of a more common order. All the prominent natural features appear with great distinctness; and by a peculiar hatching and tinting, the lava currents, heaths, moors, swamps, and other peculiarities of surface, are well shewn, among which are those enormous masses of ice (*Jökler*), of more than 200 square miles each, in different parts of the country. This really beautiful map is founded upon the government surveys, executed at the commencement of the present century, the elements of which have been subjected to a new and careful recalculation. It is accompanied by an index map on one sheet. An excellent work upon Iceland, by Dr Schleissner, published by command of the Danish Government, and presented to us by its talented author, ought also to be mentioned.—*Journal of the Royal Geographical Society*, vol. xx., 1850, Part i., p. xlviii. A copy of this very beautiful and interesting Map has just reached us.—Ed. of *Edin. New Phil. Journal.*

3. *Analysis of Gurolite, a new Mineral, by Dr T. Anderson.*—The mineral described and analysed by the author was found at Stow, in Skye, where it occurs associated with apophyllite, stilbite, and other zeolitic minerals. It is found principally in a compact basalt, different from that in which these minerals are most abundant, and which appears to have been produced by a different eruption of basaltic matter.

Gurolite occurs in the form of radiated crystalline masses with a fine lustre. It cleaves readily parallel to the plates of which the concretions are composed, and its hardness is about 3. Before the blow-pipe alone it swells up, loses water, and finally fuses with some difficulty into an opaque glass. Its analysis leads to the chemical formula $2\ (Ca\ O\ Si\ O_3) + 3\ HO$.

The author referred to the relations which this mineral bears to the other silicates of lime, of which three are already known, the names and formulæ of which are as follows:—

Wollastonite (tabular spar),	$2\ Ca\ O\ 3\ Si\ O_3$.
Kalk-trisilicat of Gjellebäck,	$Ca\ O\ Si\ O_3$.
Gurolite, . . .	$2\ (Ca\ O\ Si\ O_3) + 3\ Ho$.
Dysclasite, . . .	$3\ Ca\ O\ 4\ Si\ O_3 + 6\ Ho$.

It thus appears that gurolite is the same silicate of lime as the kalk-trisilicat, in union with water, and that its relation with dysclasite is such that two equivalents of gurolite differ from one of dysclasite by a single equivalent of lime only.

ZOOLOGY.

4. *Miseries and Delights of the Forests of Guiana.*—The task of the naturalist in Guiana—even should he escape the seasoning in Georgetown—is no holiday one, exciting though it may be to the enthusiast. Indeed, before he reaches the promised land, from some quiet nook behind the *Goldener Aue*, what dismal qualms of sea-sickness must be endured, and perilous tossing in Atlantic gales! Within a few weeks after landing, the colonial demon lays hold of him: and life is barely recovered, after a desperate struggle, when all hope had been expelled by the appearance of the "black vomit"—the first case for "twenty years," indeed, of recovery from that stage of yellow fever. The moment he enters the woods, insect terrors, exceeding in variety and fierceness the plagues of Egypt, fall upon, creep over, sting and bite him, or treacherously suck his blood while sleeping. Insidious chigoes—discovered too late—burrow in his toes, and he cannot expel them at less than the cost of every nail, and a general mangling of the flesh that lames him for months. Then there is a *bête rouge* that nestles in "softer parts," that must be driven out by sharp lemon juice and piercing needle work. Mosquitoes "murder sleep;"—nay, are they not, in some moist seasons, on the Pomeroon for instance, so fierce that not even hunger itself can resist them? He arrives after sunset, weary and fasting,—and joyfully sits down to sup on *Erythrinus Macrodon;* when lo! an army of "yellow nippers" darkens the air. Human patience cannot endure their bite,—from the untasted fish he rushes to the hammock,—in vain! They pervade all defences; nothing remains but "to quit the bed and pass the remainder of the night in raving about the room like a bedlamite! Ants are more formidable still,—because of their organization and obstinacy. Some will hang their nests loosely on impending boughs, which cannot be touched without risk of the whole enraged colony falling upon him. Woe to the botanist who, thinking no harm, reaches at some "fine specimen" of *Tillandsia,* and shakes down with it a legion of these inveterate furies! Others migrate in dense squadrons, occupying every thing in their way, driving out and eating up all insects and greens that come before them. The last is some compensation, indeed; for they clear the house of vermin,—but turn the master out of doors, the while. He calls upon some friend, and finds the hospitable man bivouacking under a tree; grieved that he cannot offer his house,—for the ants are now in it. There he sees them covering the walls like arras, and hanging in great clusters from every projection. This visitation will last, it may be, for a couple of days. But of all ants defend him from the *Ponera clavata*—a "bad one," as the Greek name implies! This creature, whose "long black body is set with detached hairs," bites like a demon.

"No words can describe the pain," should he come in its way and get a touch of its proboscis in his thumb. He feels it all over, in the breast and arms especially,—the agony is overpowering. After fruitless attempts to totter homewards, he swoons away on the road,—and passes all next day in bed, with a smart fit of irritative fever. These are a part only of the vermin and insect torments of Guiana;—we must pass on referring curious readers to Mr Schomburgk's own pages for notices of scorpions that lurk in the traveller's bedroom,—vampire bats that nightly cup his great toe, and deadly snakes. *Trigonocephalus atrox* we call one of the worst of these. His fangs are fatal, unless you cut out the bitten part on the instant. Have we not seen an Indian minus half of one foot, which he had resolutely hacked off in such an emergency, and to our surprise, he only limped a little after all?

Not alone such dangers shake the firm soul of a missionary of science in Guiana. Harder trials, perhaps, than any of these, await the man of delicate stomach, whom ethnological zeal and voluptuous forms of brown "Indian Graces," invite to friendly sojourns in the villages of *Warrans*, *Arawaaks*, or the gentle *Macusis*. They all love an intoxicating drink called *Paiwari*, for the enjoyment of which great festivals are held,—objects of the liveliest interest of course, to a student of aboriginal humanity. But, alas! the preparation of the beverage,—consisting partly in an active chewing of cassada-bread, which afterwards is left to ferment, is revolting to an unspeakable degree. You cannot help seeing the nasty process, and every new excitement of wonder and of expectation is turned into misery by loathing reflections that, before long you must drink your share of this beverage, the refusal of which would be the deadliest offence that an Indian host could receive. The brown beauties, whom you have but too well pleased with beads and bracelets, gratefully ply you with bowls from the detestable tub, until reluctant nature can hold no more, and "all joy is banished" from the inquiring soul, in spite of the wonder and drolleries of the motley scene and its novel ceremonies,—by qualms of sickening disgust. Yet the reflective mind of science is still on the alert, and does not fail to note that here at least, the reproach of having taught the Indians the vice of drinking is not justly chargeable on white men. The *Paiwari* is a "peculiar domestic institution," much older than Cortez or Raleigh.

If to its distresses we add the constant destruction of *Hortus siccus*, skins, and other rarities, by mould and other damps, and sudden decays, so that, of the collections so hardly gained, scarcely one specimen in three can at last be despatched across the Atlantic, it will result, even from this partial sketch, that the path of science in Guiana is not all hung with roses. So that more than common praise is due to one who has pursued it with constant resolution and success, in spite of these and other risks and troubles;—of perilous rapids to be mounted,—of going astray in pathless woods, where, in the rainy season, it is all but certain death to pass a night without shelter,—of cruel grasses, with sword leaves, that gash the hunter's limbs,—and giant weeds, " from ten to sixteen feet high" (*Scleria flagellans* is one of these) stinging with a fifty nettle power ;—of these, we say, and many more, which must be left to the reader's imagination.

Not that these pains are without their compensations. To the lover of nature, the spectacle of its lavish beauty, in all forms of vegetable and winged life, is a delight inexhaustible in its " infinite variety." Everything has some wondrous feature of vivacity, magnitude, or luxuriance, of which North Europeans can form no idea from descriptions, however animated. The sense is intoxicated with perfumes. Colours of glowing splendour, and countless forms of elegance or grandeur, dilate the eye with rapture. The ear is alternately ravished and perplexed with the multitudinous forest voices. The climate, in its better moods, is like the very breath of Paradise. Nor are special exhibitions wanting at times to add solemnity or provoke amazement. The blazing savannah, with its pillars of fire and smoke, rushing with a vanguard of fiery tongues over plains and hills, and drawing their contours in lines of living fire, is one of these imposing night pictures. More splendid yet is the illumination of the forest, when one of its giants (say the *Mora*, a tree 130 feet high, and 10 feet in girth) is set on fire, and the flame, roaring up through its hollow stem, gushes out above and over the upper branches, which kindle into a stupendous candelabrum ; or, when the conflagration is at its height, the whole tufted head blazing out in a *gerbe* of fire, with myriad sparks driven far on high, presents a show beside which the best pyrotechny of Europe would fade like a rushlight.

Scientific discovery, too, has its excitements and its rich rewards. Many of the choicest treasures of nations have been detected for the first time, and preserved for all time to come. The naturalist may well be envied who can feel that he has not lived in vain. Nor less useful and pleasurable is his labour in confirming doubtful points or in removing error. As an instance, we may note with especial praise an important chapter on the famous Ururi prism ; on which all doubt has been set at rest by Mr Richard Schomburgk's investigations.—*Athenæum*, No. 1215, p. 165.

HYDROLOGY.

5. *The Thunder of Waterfalls.*—During these experiments a circumstance repeatedly suggested itself, which, although a matter of as common experience as the production of bubbles, has, so far as I am aware of, hitherto escaped notice—I mean the origin of the sound of agitated water. When the smoke is projected from the

lips of a tobacco-smoker, a little explosion usually accompanies the puff; but the nature of this is in a great measure dependent on the state of the lips at the time, whether they be dry or moist. The sound appears to be chiefly due to the sudden bursting of the film which connects both lips. If an inflated bladder be jumped upon, it will emit an explosion as loud as a pistol-shot. Sound to some extent, always accompanies the sudden liberation of compressed air. And this fact is also exhibited in the deportment of our jet. If the surface of the fluid on which it falls intersects its limpid portion, the jet enters *silently*, and no bubbles, as before remarked, are produced. The moment, however, after the bubbles make their appearance, an audible rattle also commences, which becomes louder and louder as the mass of the jet is increased. The very nature of the sound pronounces its origin to be the bursting of the bubbles; and to the same cause the rippling of streams and the sound of breakers appear to be almost exclusively due. I have examined a stream or two, and in all cases where a ripple made itself heard I have discovered bubbles. The impact of water against water is a comparatively subordinate cause, and could never of itself occasion the murmur of a brook or the musical roar of the ocean. It is the same as regards waterfalls. Were Niagara continuous and without lateral vibration, it would be as silent as a cataract of ice. It is possible, I believe, to get behind the descending water at one place; and if the attention of travellers were directed to the subject, the mass might perhaps be *seen through*. For in all probability it also has its "contracted sections;" after passing which it is broken into detached masses, which, plunging successively upon the air-bladders formed by their precursors, suddenly liberate their contents, and thus create the thunder of the waterfall.—*Philosophical Magazine*, vol. i., No. 2, 4th Series.

ETHNOLOGY.

6. At the Meeting of the Royal Society of Edinburgh in January last 1851, some account was given by the Rev. J. Hannah, of an elaborate and very important paper received, through Professor Jameson, from Mr J. R. Logan of Singapore. The following is the author's own account of its nature and contents:—

Traces of an Ethnic Connection between the Basin of the Ganges and the Indian Archipelago, before the Advance of the Hindus into India; and a Comparison of the Languages of the Indo-Pacific Islanders with the Tibeto-Indian, Tibeto-Burmese, Telugu-Tamulian, Tartar-Japanese, and American Languages.

I.—*Preliminary Enquiries.*

§ 1. Principal continental connections of the Archiac ethnology of Asianesia.

§ 2. Physiological and moral evidence of an Indian connection.

§ 3. General ethnic principles and tendencies observable in the ethnology of Eastern Asia and Asianesia.

a. Mutual physiological and moral action of tribes.

b. Linguistic development and mutual action of tribes.

§ 4. Character of primordial phonology. Remnants of it in S. E. Asia.

§ 5. Cause of the transition from the monotonic to dissyllabic glossaries.

§ 6. Comparative value of structural and glossarial comparisons for ethnology. Superiority of the glossarial. Supreme importance of Phonology.

II.—*Phonetic and structural character of the Archaic languages of India.*

§ 7. Prepositional and postpositional languages.

§ 8. Character of the Tibetan and Burmese with relation to each other and to the Tartarian and S. E. Asian languages.

§ 9. The N. Gangetic or Himalayan languages.

§ 10. The S. E. Gangetic languages.

§ 11. The S. Gangetic languages.

§ 12. The Telugu-Tamulian languages.

§ 13. Comparison of the Telugu-Tamulian with the African languages.

III.—*Phonetic and structural character of the Asianesian languages.*

§ 14. Australian.

§ 15. Polynesian.

§ 15.* Papuanesian.

§ 16. S. and S. E. Indonesian.

§ 17. N. E. Indonesian.

§ 18. W. Indonesian.

IV.—*The Asianesian languages compared with the American and Tartar-Japanese languages.*

§ 19.* Asianesian compared with American languages.

§ 20.* The Asianesian compared with the Japanese, Korian, and Tungusian languages.

Sub-sect. 1. Japanese.

— 2. Korian.

— 3. Manchu.

— 4. Results.

V.—*Ethnic Glossology.*

§ 19. Principles of glossarial comparison.

§ 20. Character of Asianesian glossology.

§ 21. Permutations of sounds.
§ 22. Comparison of Definite, Segregative, and Generic words or particles.
§ 23. Pronouns.
§ 24. Numerals.
§ 25. Names of parts of the body.
§ 26. Names of Domesticated animals.
§ 27. Miscellaneous words.

Conclusion.

Several extensive extracts were read, to illustrate, *first* the relation which the author's historical views bear to those of previous inquirers in the same field; and, *secondly*, the theory, on the origin and progress of language, upon which his arguments are mainly rested.

MISCELLANEOUS.

7. *Statistics of London.*—Peace, however, also has its surveys, and grand surveys too, as well as war; and within the last few days, a remarkable instance of the peaceful class has come to hand, in the shape of two maps of London, a contemplation of which will afford food for every reflective mind.

The first of these, is a large map of the cities of London and Westminster, in the early part of the reign of Queen Elizabeth. This was presented to us by Messrs Taperell and Innes, the publishers; and, by its dimensions and characteristics, it is evidently a copy, but with additions, of the one engraved by George Vertue in 1737, for the Society of Antiquaries, "to oblige the curious of his age," from the map which then belonged to Sir Hans Sloane. The date assigned it, was 1560, on the inferential testimony of certain piers and buildings, though at first it was reported much older. London was already so large as to create an uneasiness in the royal mind, as to the effects of its probable extension; and. in the reasonings which followed, we perceive, that though calculation on precise data may be esteemed as truth in the concrete, arithmetic loosely applied to ordinary affairs may prove inexact. The fallacy of prediction on such subjects is eminently displayed in Sir William Petty's "Political Arithmetic," a work printed in 1683, after much study of statistical returns and the bills of mortality. Duly pondering over the whole results—and by the "City of London," meaning "the housing within the walls of the Old City, with the liberties thereof; Westminster, the Borough of Southwark, and so much of the built ground in Middlesex and Surrey whose houses are contiguous thereto"—he demonstrates that the growth of the metropolis must stop of its own accord before the year of grace 1800; at which period the population would, by his computation, have arrived at exactly 5,359,000. Nay, more, were it not for this stop, he shows that the increase would double in forty years, with a slight accelerating increment, as he gives the amount of human beings in the city for 1840 at 10,718,880! The identical year 1800, the commencement of a truly important century, found London still enlarging, brick-fields and scaffolding were invading all its outskirts: but the inhabitants, who had increased in a reasonably rapid ratio, numbered only 830,000.

It might here be objected, that the two plans are rather topographical than otherwise; but such a consideration does not at all invalidate the conclusions resulting from their examination. The local and limited compass embraced by topography, bears to the wide generalities of geography, the same interest and import as that which biography carries to the nationalities of history. He who is acquainted with the multitudinous details of the British metropolis, cannot therefore study the exhibition before him but with surprise. On the east, he will perceive that the Tower stands separated from London, and Finsbury and Spitalfields exhibit nothing but trees and hedge-rows; while on the west of Temple Bar the villages of Charing Cross, St Giles, and other scattered hamlets are segregated, and Westminster is a distinct city. The intervening north bank of the river Thames, or the Strand, has a line of seats and gardens of the nobility; a fact traceable in the names still remaining. At the date of this old map, London contained about 145,000 inhabitants; and was then, as now, the very focus in which the royal, the legislative, the scientific, and the trading interests of the nation were concentrated; being, as Camden said, "the epitome of all Britain, as much above the rest as the cypress is above the little sprig." In the narrative of the visit of the Duke de Nayera to the Court of Henry VIII., in 1543, London is described as one of the largest cities in Christendom, "its extent being near a league." The Thames was then the highway of the metropolis, and its single bridge a very wonder: "Never," says the Duke's secretary, admiring its beauty, "never did I see a river so thickly covered with swans as this." Paulus Jobius said, that these birds in groups greeted the arriving fleets; and one of Cardinal Pole's suite described the view of the river above bridge, as a vast mass of silver, from the abundance of swans as far as the eye could reach. How has commerce altered this?

The second of the presents mentioned is from Mr Wyld, being his latest map of London and its environs, with a novel and important addition of the levels taken by order of the Commissioners of sewers. Wonderful is the difference. We now see a very world of dwellings of 30 miles in circuit, with a population of 2,200,000 in the city and its incorporated suburbs, and their food—wheat, flesh meat, fish, vegetables, fruit, milk, wine and malt liquors—costs a million of money weekly; and to this must be added the constant circulation of cash in clothing, moveables and luxuries,—besides the enormous expenses of warming, lighting, and cleaning so vast a space. The supply of water amounts to at least 75 millions of gallons daily to about 300,000 houses; while the coals consumed are averaged at 2,000,000 of chaldrons annually; and everything is still on the increase. The present sewerage amounts to upwards of 7 millions of cubic feet on the north side of the Thames, and nearly 2½ millions on the south side. Here, then, is an extent and population sufficient to cast our old map into comparative insignificance; but London at both periods was politically the same, commanding great trade, property of every description flowing into and distributed from it, bearing an important sway in deciding the opinions of the empire at large, and arbitrating the fate of many nations.

These plans, therefore, shew—on grounds which topography renders unquestionable—how a city which so largely towered in public estimation in a former age, has swollen into its present amazing extent and splendour: being now the nucleus of the destiny of millions, and a monument to the world of the wealth, refinement, and public spirit of its inhabitants. In contemplating this remarkable growth, the mind is pleased with the advance of extent and proportionate populousness, instead of having, as in some cases, to deplore spaces once busy with the hum of men, but now utterly desolate. I have dwelt upon this, in order that Bacon's idea of the pleasure of studying—not merely looking at—a map, and the melancholy Burton's opinion also, may be understood. Indeed, such mental application, by leading to the further unveiling of the general cosmogony and phenomena of nature, contributes largely to the manifestation of the glorious plan, design, and harmonious fitness of creation.—*The Journal of the Royal Geographical Society of London*, vol. xx., 1850. Part I., p. lxx.

8. *Surgical Operations without Pain, by Papin.*—A manuscript, written by Papin, so well known for his successful experiments connected with the motive power of steam, has just been discovered (says the *Siècle*) near Marburg, a small town of Electoral Hesse. This work bears the name of "Traité des Opérations sans douleur," and in it are examined the different means that might be employed to deaden, or rather altogether to nullify sensibility when surgical operations are being performed on the human body. Papin composed this work in 1681, when filling the situation of Professor in the University of Marburg; and in it he has anticipated the effects produced in modern times by chloroform and sulphuric ether. He communicated his ideas to his colleagues in the University, but from them received any thing but encouragement. In consequence he took such a disgust to medical pursuits that he gave up his profession as a physician, and directed his attention to natural philosophy, in which he subsequently became so celebrated. On quitting Germany to return to France, he gave the manuscript to a friend of

his, Dr Börner. It at last came into the hands of a teacher named Lahn, who died near Marburg last month. It has now been purchased by the Grand Duke of Hesse for his private library.—*Illustrated London News*, No. 473, vol. xviii., p. 194.

List of Patents granted for Scotland from 18*th December* 1850 *to* 22*d March* 1851.

1. To Charles Henson, of Stepney, in the county of Middlesex, engineer, and Charles Saunderson, of the city of London, gentleman, "certain improvements in steam engines, steam boilers, and safety valves, and in apparatus and machinery for propelling vessels."—18th December 1850.

2. To John Ranson St John, of the city of New York, in the United States of America, engineer, "improvements in the construction of compasses, and apparatus for ascertaining and registering the velocity of ships or vessels through the water."—18th December 1850.

3. To James Mather, the younger, of Crow Oaks, Pilkington, in the county of Lancaster, bleacher, and Thomas Edmeston of the same place, calendarman, "certain improvements in machinery or apparatus for scouring, finishing, and stretching woollen, cotton, and other woven fabrics."—20th December 1850.

4. To Edward Dunn, of New York, but now residing at the London Coffeehouse, Ludgate Hill, in the city of London, master mariner, "an improved engine for producing motive power by the dilatation or expansion of certain fluids or gases, by the application of caloric;" being a communication.—20th December 1850.

5. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in cutting and dressing stone;" being a communication.—23d December 1850.

6. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in the manufacture of iron hurdles or fences, and of certain other articles in the construction of which wire-work is, or may be employed;" being a communication.—23d December 1850.

7. To Thomas Allan, of Glasgow, in the county of Lanark, North Britain, iron-founder, "certain improvements in paving or covering roads, streets, and other surfaces of a similar nature."—23d December 1850.

8. To William Hodgson Gratrix, of Salford, in the county of Lancaster, engineer, "certain improvements in the method of producing or manufacturing velvets or other pile fabrics."—24th December 1850.

9. To James Naysmith, of Patricroft, in the county of Lancaster, engineer, and John Barton, of Manchester, in the same county, copper-roller manufacturer, "certain improvements in machinery or apparatus for printing calicoes and other surfaces, and also improvements in the

manufacture of copper or other metallic rollers to be employed therein, and in the machinery or apparatus connected with such manufacture."—24th December 1850.

10. To James Forster, of Liverpool, merchant, "improvements in filtering water and other liquids."—31st December 1850.

11. To Francis Edward Colegrave, of Brighton, in the county of Sussex, Esq., "improvements in the valves of steam and other engines, in causing the driving-wheels of locomotive engines to bite the rails, and also in supplying water to steam-boilers."—31st December 1850.

12. To Thomas Brown, of Muscovy Court, Tower Hill, in the city of London, gentleman, "improvements in machinery for raising and lowering weights."—31st December 1850.

13. To Edward d'Orville, and John Partington, of Manchester, in the county of Lancaster, manufacturers, "certain improvements in finishing thread or yarn."—31st December 1850.

14. To James Hill, of Stalybridge, in the county of Chester, cotton-spinner, "improvements in, or applicable to, certain machines for preparing cotton, wool, and other fibrous substances for spinning and doubling."—3d January 1851.

15. To Henry Bessemer, of Baxter House, St Pancras Road, in the county of Middlesex, engineer, "certain improvements in apparatus acting by centrifugal force, in the manufacture of sugar, and other improvements in the treatment of saccharine matter by such apparatus."—6th January 1851.

16. To Lucien Vidie, of 14 Rue du Grand Chantier, Paris, in the republic of France, French advocate, "certain improvements in measuring the pressure of air, steam, gas, and liquids."—8th January 1851.

17. To John Coope Haddan, of Bloomsbury Square, in the county of Middlesex, engineer, "improvements in the manufacture of railway carriages and of railway wheels, and also of panels for carriages, and other purposes."—9th January 1851.

18. To Samuel Hall, late of Basford, near Nottingham, engineer, "improvements in the manufacture of starch and gums, and in furnaces and steam-boilers, with safety apparatus to be used in such manufactures, and for other purposes."—10th January 1851.

19. To John Corry, of Belfast, in the kingdom of Ireland, damask manufacturer, "improvements in the machinery or apparatus for weaving figured fabrics, which machinery or apparatus is also applicable to other purposes for which Jacquard apparatus is or may be employed."—13th January 1851.

20. To John Ranson St John, of the city and state of New York, in the United States of America, engineer, "improvements in the process of, and apparatus for, manufacturing soap;" being a communication.—15th January 1851.

21. To Joseph Gibbs, of Devonshire Street, in the county of Middlesex, civil-engineer, "improvements in the manufacturing paints and cements, and panels or surfaces on which paints or cements are to be, or

may be applied, part of which improvements are applicable to other useful purposes."—20th January 1851.

22. To John Clarkson Milns and Samuel Pickstone, of Radcliff Bridge, in the county of Lancaster, manufacturers, "certain improvements in machinery or apparatus used in spinning, doubling, and weaving cotton, flax, and other fibrous substances."—20th January 1851.

23. To Edward Clarence Shepard, of Parliament Street, in the city of Westminster, gentleman, "certain improvements in electro-magnetic apparatus, suitable for the production of motive power of heat and of light;" being a communication.—22d January 1851.

24. To James Slater and John Nuttall Slater, of Dunscar, near Bolton-le-Moors, in the county of Lancaster, bleachers, "certain improvements in machinery or apparatus for the purpose of stretching and opening textile or woven fabrics."—23d January 1851.

25. To James Hamilton, of London, engineer, "improvements in machinery for sawing, boring, and shaping wood."—23d January 1851.

26. To Julian Bernard, of Green Street, Grosvenor Square, in the county of Middlesex, gentleman, "improvements in the manufacture or production of boots and shoes, and other articles made of leather, dressed skins, or other materials, and in the materials and machinery or apparatus to be employed therein;" being a communication.—24th January 1851.

27. To Richard Archibald Brooman, of the firm of Messrs J. C. Robertson and Company, of 166 Fleet Street, in the city of London, patent agents, "certain improvements in steam machinery and apparatus connected therewith;" being a communication.—24th January 1851.

28. To Richard Archibald Brooman, of the firm of Messrs J. C. Robertson and Company, of 166 Fleet Street, in the city of London, patent-agents, "an improvement or improvements in abdominal supporters;" being a communication."—24th January 1851.

29. To Charles de Bergue, of Arthur Street West, in the city of London, engineer, "improvements in, and in the construction of, the permanent way of railways."—27th January 1851.

30. To Samuel Clift, of Bradford, near Manchester, manufacturing chemist, "improvements in the manufacture of muriatic acid, soda, potash, and glass, and of chlorine."—27th January 1851.

31. To William Beckett Johnson, of Manchester, in the county of Lancaster, manager for Messrs Ormerod and Son, engineers, "certain improvements in steam-engines, and in apparatus for generating steam; such improvements in engines being wholly or in part applicable where other vapours or gases are used as the motive power."—29th January 1851.

32. To Samuel Morand, of Manchester, "improvements in apparatus used when stretching and drying fabrics."—29th January 1851.

33. To William M'Gavin, of Glasgow, in the county of Lanark, North Britain, miller, "certain improvements in steam-boilers, and furnaces, and fire-places, and in the prevention of smoke."—29th January 1851.

34. To Edward David Ashe, of Brompton, in the county of Middlesex, Lieutenant in Her Majesty's royal navy, "a new or improved nautical instrument or instruments applicable especially, amongst other purposes, to those of great circle sailing."—29th January 1851.

35. To Joshua Horton, of Ætna Works, Smeltwick, in the county of Stafford, steam-engine boiler and gas-holder manufacturer, trading under the firm or style of Joshua and William Horton, "improvements in the construction of gas-holders."—30th January 1851.

36. To Peter Fairbairn, of Leeds, in the county of York, machinist, and John Hetherington, of Manchester, in the county of Lancaster, machinist, "certain improvements in mouldings for casting pipes, railings, gates, agricultural implements, and other metal articles, and also in preparing patterns or models for the same."—31st January 1851.

37. To John Stopperton, of the Isle of Man, engineer, "certain improvements in propelling vessels, part of which improvements are applicable to steam-engines and pumps."—31st January 1851.

38. To Benjamin Rotch, of Lowlands, in the county of Middlesex, Esq., "a factitious saltpetre, and a mode by which factitious saltpetre may be obtained for commercial purposes."—3d February 1851.

39. To Nathaniel Jones Amies, of Manchester, in the county of Lancaster, manufacturer, "certain improvements in the manufacture of braid, and in the machinery or apparatus connected therewith."—3d February 1851.

40. To Frederick Watson, of Moss Lane, Hulme, Manchester, in the county of Lancaster, gentleman, "improvements in sails, rigging, and ships' fittings, and machinery and apparatus employed therein."—3d February 1851.

41. To James Webster, of Leicester, engineer, "improvements in the construction, and means of applying carriage and certain other springs."—5th February 1851.

42. To Henry Beesemer, of Baxter House, in the county of Middlesex, civil-engineer, "certain improvements in the sugar-cane press."—6th February 1851.

43. To Selim Richard St Clair Massiah, of Alderman Walk, New Broad Street, in the city of London, "improvements in the manufacture of artificial marble and stone, and in treating marble and stone."—7th February 1851.

44. To Joseph Shaw, of Paddock, near Huddersfield, in the county of York, cloth finisher, "improvements in constructing and working certain parts of railways."—7th February 1851.

45. To Francis Clark Monatis, of Earlston, in the county of Berwick, Scotland, builder, "an improved hydraulic syphon."—7th February 1851.

46. To Richard Stuart Norris, of Warrington, in the county of Lancaster, civil-engineer, "certain improvements in the construction of the permanent way of railways, bridges, locks, and other erections, wholly, or in part, constructed of metal; also improvements in breaks for railway carriages."—10th February 1851.

47. To William Weild, of Manchester, in the county of Lancaster, engineer, "improvements in machinery for turning and burnishing."—10th February 1851.

48. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, "improvements in machinery or apparatus for producing ice, and for general refrigeratory purposes;" being a communication.—11th February 1851.

49. To Ewald Reipe, of Finsbury Square, in the city of London, merchant, "improvements in refining steel."—12th February 1851.

50. To Peter Clausson, of Cranbourne Street, in the county of Middlesex, gentleman, "certain improvements in bleaching, in the preparation of materials for spinning and felting in yarns and felts, and in the machinery employed therein;" part of which improvements have been communicated to him by a foreigner residing abroad.—12th February 1851.

51. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in manufacturing looped and other woven fabrics;" being a communication.—14th February 1851.

52. To Charles Gotthelf Kind, of Paris, in the republic of France, engineer, and Charles Alexis de Windel, iron-master, also of Paris in the republic of France, "improvements in the process and instruments to be used for boring the earth, and sinking shafts of any given diameter, for mining and other purposes, and in the means of lining such shafts."—14th February 1851.

53. To James Thomson Wilson, of Stratford-le-Bow, in the county of Middlesex, chemist, "improvements in the manufacture of alum, and in obtaining ammonia."—14th February 1851.

54. To David Ferdinand Masnata, of Golden Square, Regent Street, in the county of Middlesex, gentleman, "a new mechanical system with compressed air adapted to obtain a new moving power."—17th February 1851.

55. To William Burgess, of Newgate Street, in the city of London, gutta percha dealer, "improvements in machinery for cutting turnips and other substances."—17th February 1851.

56. To Thomas Wickstead, of Old Ford, in the county of Middlesex, civil-engineer, "improvements in the manufacture of manure."—19th February 1851.

57. To Bennet Woodcroft, of Furnivals Inn, "improvements in machinery for propelling vessels."—21st February 1851.

58. To Adolphus Oliver Harris, of High Holborn, in the county of Middlesex, philosophical instrument maker, "improvements in barometers;" being a communication.—26th February 1851.

59. To Joseph Crossley, of Halifax, carpet manufacturer; George Collier, of the same place, mechanic; and James Hudson, of Littleborough, printer, "improvements in printing yarns for and in weaving carpets, and other fabrics."—3d March 1851.

60. To George Smith, of Manchester, in the county of Lancaster,

engineer, "certain improvements in steam-engines, and also improvements in feeding or supplying the boilers of the same, part or parts of which improvements are also applicable to other similar purposes."—4th March 1851.

61. To John Hetherington, of Manchester, in the county of Lancaster, machinist, "improvements in machinery for preparing, spinning, and manufacturing fibrous substances."—4th March 1851.

62. To Alfred Cooper, of Rumsay, in the county of Hants, grocer, "improvements in steam and other power engines, and in the application thereof to motive purposes; also in the method of, and machinery for, arresting or checking the progress of locomotive engines and other carriages."—5th March 1851.

63. To Henry Richardson, of Aber, Hernaut Bala, North Wales, Esquire, "certain improvements in life-boats."—7th March 1851.

64. To William Stones, of Queen Hythe, in the city of London, stationer, "improvements in the manufacture of safety paper for bankers' cheques, bills of exchange, and other like purposes."—7th March 1851.

65. To Joseph Baldwin and George Collier, mechanics, and Joseph Crossley, all of Halifax, "improvements in the manufacture of carpets and other fabrics."—12th March 1851.

66. To George Roberts, of Selkirk, in the kingdom of Scotland, manufacturer, "an improved manufacture of certain yarns of linen, wool, silk, cotton, and other fibrous substances."—13th March 1851.

67. To Samuel Brisbane, of Manchester, in the county of Lancaster, pattern-maker, "certain improvements in looms for weaving."—14th March 1851.

68. To George Guthrie, of Appleby, chamberlain to the Earl of Stair, and residing at Rephad, by Stranraer, in the county of Wigtown, "improvements in machinery for digging, tilling, or working land."—14th March 1851.

69. To Richard Archibald Brooman, of the firm of J. C. Robertson and Company, of 166 Fleet Street, in the city of London, patent-agents, "improvements in purifying water and preparing it for engineering, manufacturing, and domestic purposes;" being a communication.—17th March 1851.

70. To William Eccles, of Walton-le-Dale, in the county of Lancaster, cotton-spinner, "certain improvements in looms for weaving."—17th March 1851.

71. To Edward Lloyd, of Dee Valley, in the county of Merioneth, North Wales, engineer, "certain improvements in steam-engines, which improvements are, in part, or on the whole, applicable to other motive power."—17th March 1851.

72. To Herbert Taylor, of 46 Cross Street, Finsbury, in the county of Middlesex, merchant, "certain improvements in the manufacture of carbonates and oxides of barytes, and oxides of barytes and strontia, sulphur, or sulphuric acid, from the sulphate of barytes and strontia, and for consequent improvements in the manufacture of carbonates and oxide of potash and potash;" being a communication.—19th March 1851.

INDEX.